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MDCCCLVI.

ST. GEORGE'S HOSPITAL
MEDICAL AND SURGICAL SCHOOL.
Session 1856—57.

Physicians—Dr. WILSON, Dr. NAIRNE, Dr. PAGE, and Dr. BENCE JONES, F.R.S.
Obstetric Physician—Dr. ROBERT LEE, F.R.S.
Assistant-Physicians—Dr. PITMAN and Dr. FULLER.
Surgeons—Mr. CÆSAR HAWKINS, Mr. CUTLER, Mr. TATUM, & Mr. H. CHARLES JOHNSON.
Assistant-Surgeons—Mr. PRESCOTT HEWETT and Mr. G. D. POLLOCK.

HOSPITAL PRACTICE.—The Hospital contains 350 Beds.
Attendance of the Physicians and Surgeons daily, at One o'clock.
Surgical Operations on Thursdays, at One o'clock.

A MATERNITY DEPARTMENT, for the delivery of married lying-in women at their own homes, is established at the hospital, under the superintendence of the Obstetric Physician.

CLINICAL LECTURES are given during the Winter and Summer Sessions.

CLINICAL INSTRUCTION on the Diseases peculiar to Women is given in the Wards of the Hospital by the Obstetric Physician.

PRACTICAL PHARMACY, under the Superintendence of the Apothecary of the Hospital.

The Pupils attending the Medical Practice may become, when qualified, Clinical Clerks to the several Physicians.—The Pupils attending the Surgical Practice may become, when qualified, Clinical Clerks to the several Surgeons.

Pupils entering to the Surgical Practice for Twelve Months are allowed, when qualified, to dress the Patients for Three Months, and Perpetual Pupils for Six Months, without additional fee. The Dresser of the Surgeon of the Week boards at the Hospital free of expense.

The Perpetual Pupils are eligible to be Assistant House-Surgeon for Six Months, and House Surgeon for Twelve Months (without additional fee), when properly qualified for the office.

THE WINTER COURSE OF INSTRUCTION WILL COMMENCE ON TUESDAY, OCTOBER 1ST.

Gentlemen becoming pupils of this Hospital who desire to be Members of the Royal College of Surgeons, or Licentiates of the Society of Apothecaries, may qualify themselves for Examination on paying 40 Guineas at the commencement of the first year, 40 Guineas at the commencement of the second year, and 12 Guineas at the commencement of the third year. The payment for the year will admit the pupil to all the Lectures, and to the Hospital Practice required, and for that year only.

Special entries to Hospital Practice, or to any separate Course of Lectures, may be made as heretofore.

SCHOLARSHIPS, PRIZES, &c.

At the end of the Summer Session, Examinations will be held for Three Exhibitions: one of 20*l.* and one of 40*l.* for Students in their first year, and one of 50*l.* for a Student in his second year; and at the end of each course of Lectures, Examinations take place of those students who are candidates for Prizes and honorary distinctions. A Prize for the best Clinical Report of Surgical Cases is given by Sir Benjamin Brodie, and the Thompson Medal will be awarded for the best Clinical Report of cases observed in the Hospital. Sir Charles Clarke's Prize for good conduct will be awarded to the student of the Hospital whose general good conduct most entitles him to this distinction.

Two Curators are appointed annually by the Weekly Board, on the recommendation of the Medical School Council, with a salary of 50*l.* each per annum. Two Registrars are appointed annually by the Weekly Board, on the recommendation of the Medical School Council, each with a salary of 20*l.* per annum.

An Obstetric Assistant is appointed by the Weekly Board, and resides in the Hospital.

Some of the Lecturers and other gentlemen connected with the Hospital receive students to reside with them.

Further information may be obtained from Mr. G. D. POLLOCK, the Treasurer of the School; from any of the Lecturers; or from the Apothecary of the Hospital, who is authorized to enter the names of Students.

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THE BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

JULY, 1856.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

1. *Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals.* Delivered at the Royal College of Surgeons. By RICHARD OWEN, F.R.S., Hunterian Professor to the College. Second Edition. Illustrated by numerous Woodcuts.—*London*, 1855. 8vo. pp. 689.
2. *General Outline of the Organization of the Animal Kingdom, and Manual of Comparative Anatomy.* By THOMAS RYMER JONES, F.R.S., Professor of Comparative Anatomy in King's College, London, &c. Second Edition. Illustrated by Four Hundred Engravings.—*London*, 1855. 8vo. pp. 842.

IT is a somewhat singular coincidence, that the only two British Treatises on Comparative Anatomy which can be pointed-to as in any degree representing the present state of that science, and which made their first appearance almost contemporaneously, should have reached a second edition at the same time. The 'Lectures' of Professor Owen originally appeared as the notes of Mr. White Cooper, revised by the Professor himself; and were published in numbers as they were delivered, the completed volume bearing the date 1843. The 'General Outline' of Professor Rymer Jones also originally appeared in numbers, and the complete volume bears date 1841. Having published in the interval the first volume of his 'Comparative Anatomy of the Vertebrata'—the 'Anatomy of Fishes,'—and intending, as we trust, to complete that great work in a manner worthy of his unrivalled knowledge of Vertebrated Animals, Professor Owen has thought it desirable to re-issue his 'Lectures on the Invertebrata' in an enlarged form; adding a considerable quantity of new matter, and inserting numerous references to the original authorities for the facts and opinions advanced; and taking, of course, the entire responsibility upon him-

self. We are led to suppose that the author considers himself, in these additions, to have brought his work up to the existing state of knowledge on this subject; and he advances no reason why it should not be fairly tried by the standard which he has himself raised.—Professor Rymer Jones, on his part, after referring to some of the most important advances which have been made by Continental and British labourers in this field since the publication of his previous edition, tells us that he "has endeavoured, to the best of his ability, to keep pace with their diligence and onward progress, so as adequately to record and acknowledge their contributions to the general stock of scientific lore."

It will be our duty, and, we are sorry to say, our by no means pleasing duty, to inquire how far the additions and alterations made in these Treatises can be considered as placing them upon the level of our existing knowledge of the subject of which they treat. In doing this, however, we must limit ourselves to one particular department,—the group of animals constituting the Radiata of Cuvier;—since it will be necessary for us, in order to show our readers what are our grounds of complaint, to enter into some preliminary detail as to the present aspect of the border-ground between the Animal and Vegetable kingdoms, which will not, we hope, be unacceptable to them. It is, of course, to the first of the works before us that we shall more particularly address ourselves. Professor Owen occupies a position second to none in that department of Comparative Anatomy which he has made more particularly his own—viz., the Osteology of the Vertebrata; and considering the vast advances which the Comparative Anatomy and Physiology of the Invertebrata have made of late years, and the large number of labourers whose contributions must be individually studied in order to embody them in any general system, we opened the work with a misgiving that his acquaintance with these would be found to be far from complete, and that many old errors would be retained, many new truths passed by. It is quite enough, indeed, for one man's work, to keep pace with the rapid progress of any single department of this science: and Siebold, one of the most laborious and conscientious of continental systematic writers, as well as himself one of the most accurate and zealous of original inquirers, exercised a wise discretion in restricting himself, in the production of his admirable 'Vergleichende Anatomie,' 1848,* to the Invertebrated classes; the anatomy of the Vertebrata being undertaken by his colleague Stannius. It would be well if men of great ambition and comprehensive grasp of mind would act more upon the time-honoured adage, "*Non omnia possumus omnes.*" The authority which they acquire by their labours in one department, gives them a *prestige* in regard to any others that they may undertake to elucidate, which becomes mischievous, when, as too often happens, they do their work imperfectly by grasping at too much. Professor Owen's authority is deservedly so high on the subjects which he has made his own, that we are most desirous for his sake that he should not lower it by striving for what it is impossible in the nature of things that he can thoroughly attain; and we feel called upon to watch with a careful eye

* An American translation of this masterly work, with numerous Notes and additional References, by the late Dr. W. J. Burnett, bringing it down to 1854, is, in our opinion, by far the best Treatise on Invertebrate Anatomy, that the English student can have recourse to.

that it be not employed to the positive detriment of science, in perpetuating error and retarding the progress of truth.

The aim of Professor Rymer Jones's treatise is less high, and his authority is of less weight; but from its popular style, the generally-good selection of its subjects, and the beauty of its illustrations, it has acquired a reputation amongst students, of which we should gladly see it rendered more worthy.

A good-natured criticism of such works, in which their excellences alone should be dilated-on, and their defects altogether passed by or scarcely noticed, would be much more agreeable to ourselves, as well as more palatable to the subjects of it. But we must speak out as the interests of truth and justice seem to us imperatively to demand, whatever be the cost to ourselves.—With these preliminary remarks, we enter upon that general survey of the lowest forms of Animal life, which will enable us to inquire how far their nature and meaning have been understood by our authors.

The association of a large assemblage of these forms under the designation *Protozoa*, as first proposed (we believe) by Siebold, has come to be very generally accepted among the Zoologists and Physiologists of Germany, although there is not yet a complete accordance as to the definition of the group, and the range of forms which it should include. The definition given by Siebold stands as follows:—"Animals in which the different systems of organs are not distinctly separated, and whose irregular form and simple organization are reducible to the type of a cell." The fundamental idea contained in this definition was more fully expanded by him in a valuable essay 'On Unicellular Plants and Animals,' published in the first volume of Siebold and Kölliker's 'Zeitschrift;' wherein he discusses the relations which his Protozoa bear to the *Protophyta* that constitute the parallel group in the Vegetable kingdom, and inquires into the validity of the characters which have been assigned as the basis of their separation. Although the correctness of many of the details contained in that essay has been disproved by subsequent research, and although the general doctrine of *cells* in vogue at the time of its production has been conclusively shown to require revision,* yet the fundamental idea still remains unshaken—viz., that there is a division of the Animal kingdom, among the members of which there is no more differentiation of organs than there is in the simplest Plants, and which in this respect correspond to the earliest embryonic states of the higher animals. Whether this division may be fairly considered to have been permanently established, or whether it must be regarded for the present as provisional only, is a question which we shall be in a better position to discuss, when we shall have examined some of the principal facts that bear upon it.

The general result of recent microscopic investigation, in regard to the lowest forms of Vegetable and Animal life, seems to us to lead to this conclusion—that organisms may possess an independent existence, may go through all the phenomena of growth, multiplication, and reproduction, and may even possess considerable power of spontaneous motion, without having advanced even so far in the differentiation of their parts as to

* See Mr. Huxley's paper in the British and Foreign Medico-Chirurgical Review, vol. xii. p. 285.

possess those attributes which are involved in the ordinary idea of a "cell." By way of explaining our meaning, we shall select an illustration from each kingdom; and the comparison of the two will enable us to inquire in what lies the essential difference between them.

One of the humblest of known Protophytes—the *Palmoglæa macrococca* (Kützing), whose multiplication gives origin to the green slime that is found on damp stones and walls,—consists of isolated particles of a spheroidal shape and greenish colour, commonly imbedded in a stratum of gelatinous matter, which an ordinary observer would at once pronounce to be vegetable cells. But a careful examination shows that there is here no definite distinction between "cell-wall" and "cell-contents;" the whole particle being composed of a nearly-homogeneous mass of "protoplasm," through which chlorophyll-granules are dispersed. In the midst of these, however, a nucleus may be sometimes discerned; and this is usually brought into clear view by the action of tincture of iodine, which turns the nucleus dark-brown. These particles, increasing in size, undergo duplicative subdivision by the usual process of elongation and constriction; and it is observable that the nucleus gives indications of the commencement of this subdivision earlier than the particle which encloses it. Each new cell (if such it may be called) then begins to secrete from its surface a gelatinous envelope of its own; so that, by its intervention, the two are usually soon separated from one another. Sometimes, however, this is not the case; the process of subdivision being so quickly repeated, that there is not time for the production of a separate gelatinous envelope to each particle; so that a series of spheroids, hanging-on one to another, is produced. There appears to be no definite limit to this kind of multiplication; and extensive areas may be quickly covered, in circumstances favourable to the nutrition of the plant, by the products of the duplicative subdivision of one primordial cell. This, however, is simply an act of *growth*, precisely analogous to the multiplication of cells in the earliest embryonic condition of the higher Plants and Animals, before any differentiation of organs begins to show itself. And as every cell thus produced is similar to every other, and may live independently of it, such plants may still be appropriately designated as "unicellular," notwithstanding that they may be composed of large aggregations of cells connected by a gelatinous matrix, instead of being mere agglomerations of cells completely isolated from each other. The Palmoglæa not only thus grows and multiplies, but it also performs what is now coming to be generally recognised as a true *generative* process; which takes place, as might be expected, on the simplest of all types. For this process consists in the *conjugation* of any pair of cells, the substance of the two undergoing a complete mutual fusion, which is not obstructed by the intervention of any limiting membrane; the communication is usually made at first by a narrow neck or bridge, and gradually extends through a large part of the contiguous boundaries, until at last the whole of each particle is involved in it. A "spore" is thus formed, which is the "primordial cell" of a new generation, and which gradually evolves itself into an aggregation resembling that out of which it arose, by a renewal of the process of duplicative subdivision. This spore is something very different, both in aspect and in composition, from the body

that would be produced by the mere coalescence of two particles; for the green granular matter disappears, its place being taken by oil-particles, which are at first small and distant, but gradually become larger and coalesce so as to form oil-drops; and the colour of the body changes, with the advance of this process, from green to a yellowish-brown. When the spore begins to vegetate, on the other hand, producing a pair of new cells by binary subdivision, a converse change occurs: for the oil-globules disappear, and green granular matter takes their place, whereby the ordinary colour of the plant is restored. This is precisely analogous to what occurs in the maturation and germination of the seed among higher plants; and the analogy is rendered yet more complete by the fact, that the spore, like the seed, is capable of remaining dormant for an unlimited period, when deprived of moisture.

Now for such a mass of protoplasm to become converted into what is ordinarily regarded as the type of the Vegetable cell, a series of changes must take place in it, involving a differentiation between the cell-wall and the cell-contents; and this involves, on the one hand, a greater consolidation of the external layer of the protoplasm, and a more complete liquefaction of its internal portion. The membrane that is first formed, which has been termed by Mohl the "primordial utricle," is identical in composition with the albuminous protoplasma, as is shown by the effects of re-agents; and it does not always seem distinctly separable from the layer of protoplasm by which it is lined. Some recent Vegetable Physiologists, indeed, question its proper existence, affirming that it is merely the superficial layer of protoplasm, more tenacious than the rest. But to us it appears that, looking to the origin and nature of this membrane, the question is simply one of *degree* of differentiation. When the external layer, call it what we may, has such a tenacity that the substance of two cells brought together cannot coalesce without a rupture of this integument, we must call it a membrane, even though it may differ but very little from the viscid matter it surrounds.—The typical cell, if isolated, subsequently acquires a complete envelope of cellulose, secreted from the surface of the primordial utricle; but this, which is commonly known as the cell-wall in Vegetable cells, seems to have no other than a protective function; and where the cells are packed closely into a parenchyma, their cellulose-walls coalesce, like the gelatinous envelopes of the particles of Palmoglœa which are homologous with them, so that the boundaries of those proper to individual cells cannot be distinguished. Now whilst this process of consolidation is taking place externally, a reverse change, that of liquefaction, is in progress within. This commences by the formation of *vacuoles* in the substance of the protoplasm; these, however, not being empty spaces, but cavities filled with a fluid more watery than the protoplasm. These "vacuoles" increase in number and in size, the smaller ones coalescing to form larger; and at last they come to occupy nearly the whole interior of the cell, the primordial utricle being still lined by a layer of viscid protoplasm, to which the colouring matter is usually in great degree restricted, although this is sometimes diffused through the whole cell-contents. And thus the typical Vegetable cell comes to consist of—1. The cellulose wall; 2. The primordial utricle; 3. A layer of protoplasm in contact with it; 4. The

watery cell-sap of the interior; and 5. The nucleus, usually imbedded in the protoplasm-layer;—these parts being developed, by a process of gradual "differentiation," out of a minute mass of protoplasm, in which the nucleus was the only part to be separately distinguished.

The successive stages of this formation may be best traced-out by careful observation of the process of cell-growth in the higher Algæ; but the study of the development of new organs in Phanerogamic Plants leads to the same conclusions; and the results at which Mr. Wenham* has lately arrived, from observations chiefly made on the newly-imported aquatic weed, *Anacharis alsinastrum*, are so instructive that we shall subjoin a brief summary of them. He finds that when a new leaflet is being formed from the main stem, it commences, not (as is commonly supposed) in a single cell, but in the simultaneous development of some hundred at once, which make their first appearance in the midst of a mass of protoplasm which is enclosed in a membrane that subsequently seems to become the epidermis of the leaf. This mass is at first homogeneous; but it is soon seen to contain a multitude of cavities of irregular size and shape, filled with liquid, whilst the protoplasm between these becomes more viscid. The number of these is often increased, and their size rendered more uniform, subsequently to their first formation, through the division of the larger cavities into two by the interposition of a narrow bridge of protoplasm, or into three by the interposition of a broader bridge, in the substance of which another cavity developes itself: whilst new cavities appear wherever there is any considerable accumulation of protoplasm not already hollowed-out. These cavities are next observed to be lined with a definite membrane; and within this, protoplasm, chlorophyll, and cyclosis-currents subsequently become distinguishable.

On the importance of the independent support afforded by these observations to the doctrine of Mr. Huxley already referred-to, it is quite unnecessary for us to enlarge; and we shall only remark that not only are we fully satisfied of the competence and fidelity of Mr. Wenham as an observer, but his view harmonizes with a number of facts which have fallen under our own cognizance, and which the ordinary doctrines of cell-development have not served to explain.

Turning now to the Protozoa, we find in the *Amœba* and in the *Actinophrys*, types of animal existence, which, in so far as we are yet acquainted with them, may be legitimately ranked on the same level as the Palmoglœa, although placed on the other side of the boundary line, for reasons which will presently be apparent. The body of each of these creatures is a minute mass of a substance which long since received from Dujardin the appropriate name of "sarcode," and which seems to be the equivalent of the protoplasma of the Protophyta; resembling it very closely in chemical composition and in general attributes, but being endowed in addition with a high degree of contractility. The body is not enclosed, in either of these beings, by a distinct limitary membrane, although the outer stratum of the sarcode obviously possesses more consistence than its inner part, the latter being semifluid. Vacuoles or clear spaces are seen in various parts of the sarcode-body; and in these are very commonly observable alimentary particles, introduced in the way to be presently

* Transactions of the Microscopical Society, 1856, p. 1 et seq.

described. Besides these vacuoles, a "contractile vesicle," which pulsates at tolerably-regular intervals, is always to be distinguished; sometimes in the interior of the body, sometimes near its surface, and sometimes projecting above its surface. The chief difference between Amœba and Actinophrys, which agree in the foregoing particulars, lies in the nature of the changes of form which both of them exhibit, and in the mode in which food is received through their means. In the Amœba, the contour of the whole body is continually undergoing change; for the shapeless mass puts-forth one or more finger-like prolongations, which are simply extensions of its sarcode-substance in those particular directions; and a continuation of the same action, first distending the prolongation, and then (as it were) carrying the whole body into it, causes the entire mass to change its place. After a short time, another prolongation is put forth, either in the same or in some different direction; and the body is again absorbed into this. When the creature, in the course of its progress, meets with a particle capable of affording it nutriment, its sarcode-body spreads itself over this, so as to receive it, through any part of its parietes, into some of the vacuoles in its interior; a sort of stomach being thus extemporized, within which the alimentary particle undergoes a sort of digestion, the nutrient material being extracted by the enveloping sarcode, and any indigestible part making its way to the surface, and finally escaping through any part of it with which it happens to be in proximity. The form of the Actinophrys, on the other hand, never seems to depart widely from the globular; but its sarcode usually extends itself into a great number of contractile filaments, termed *pseudopodia*. The number and length of these, however, are continually varying; and sometimes they entirely disappear, in which case the animal cannot be certainly distinguished from an Amœba, until it begins again to put them forth. It is by the agency of these filaments, that the food of the creature is obtained; for whilst the body remains at rest, the pseudopodia act the part of the tentacula of the Hydra, being so many fishing-lines which are ready to entrap any suitable particles that may come in their way; and not merely comparatively inert and lowly-organized beings, but various small animals of great activity as well as high organization, are thus laid-hold of. When any such body happens to come into contact with one of the pseudopodial filaments, this usually retains it by adhesion, and forthwith begins to retract itself; as it shortens, surrounding filaments also apply themselves to the captive particle, bending their points together so as gradually to enclose it, and then themselves shortening progressively until the prey is brought to the surface of the body. That the threads of sarcode of which the pseudopodia are composed, are not furnished (any more than the body itself) with an investing or limitary membrane, is shown by their complete coalescence or fusion with each other, when they happen to come into mutual contact. The food thus drawn to the surface of the body by the contractility of its pseudopodial extensions, is introduced into its substance by the continuance of the same kind of operation, and gradually passes from its peripheral to its central part, where its digestible portion undergoes solution, the indigestible part (such as the shell of a minute Crustacean, or the hard case of a Rotifer) finding its way out, as in the Amœba, through any part of the surface of the body.

Thus in these creatures, although they have neither digestive cavity, mouth, nor anus,—although they are to all appearance nothing else than particles of animated jelly not even confined within a definite membrane, —the prehension and ingestion of food, the extraction of its nutritive portion by a digestive process, and the rejection of what cannot be thus reduced, by an act of defecation, are performed as characteristically, and in reality as perfectly, as in the highest animals. They multiply, however, after the manner of Protophytes, by self-division; and it has been found, in the Amœba, that portions separated from the sarcode-body, either by cutting or tearing, can develope themselves into independent beings. It has been thought, too, that, as in the Protophyta, their generative function consists in an act of "conjugation;" but recent observations have shown that it is by no means unfrequent for two, three, or even more individuals to coalesce together for a time, without the formation of any product at all analogous to the vegetable spore; the compound body afterwards separating again into detached individuals, which do not, however, always present the relative sizes they had before the occurrence of this curious fusion. Hence we must confess ourselves ignorant at present of this essential part of the life-history of these Protozoa; and neither can it be predicated in what their Generative operation is likely to consist, nor have we any idea of the nature of its product. It is quite possible, from the analogy of other low forms of animal organization, that after multiplying almost indefinitely in the Amœba- or the Actinophrys-form, some entirely different form may evolve itself, with which we may be already acquainted, though without entertaining the least suspicion of its relationship to this group: by this the generative operation may be performed, and its first products may be Protozoa of the one or of the other kind respectively. Or it would also be consistent with what we see elsewhere, that this generative act should be performed in the Amœba- or the Actinophrys-condition, and that its product should be an animal of some very different organization, which in its turn reproduces the Amœba- or Actinophrys-type by an act of gemmation. We dwell upon the deficiency of our knowledge on this point, and on the possible contingencies of the solution, to show how little we yet know about these curious creatures; and thus, on the one hand, to prevent their place in the scale from being considered as definitely fixed, and, on the other, to stimulate and direct further observation.

If, now, we compare an *Amœba* or an *Actinophrys* in its quiescent state, with a *Palmoglœa*, or any equally simple Protophyte, we can scarcely assign any *structural* characters by which one could be differentiated from the other. But when we look at their *physiological* actions, how wide is the distinction. The Protophyte, like the Phanerogamic plant, obtains the materials of its nutrition from the air and water that surround it, and possesses the marvellous power of detaching oxygen, hydrogen, carbon, and nitrogen from their previous binary combinations, and of uniting them into chlorophyll, starch, albumen, and other ternary and quaternary combinations: but the Protozoon, in common with the highest members of the Animal kingdom, is (to all appearance) destitute of any such combining power; and is consequently dependent for its support upon organic substances previously elaborated by other beings; so

that it must in the end derive its sustenance, directly or indirectly from the Vegetable kingdom. Again, the Protophyte obtains its nutriment by the absorption of liquid and gaseous molecules, which penetrate its body by simple imbibition: whilst the Protozoon, though destitute of any permanent mouth, stomach, intestine, or anus, extemporizes (so to speak) all these organs for itself whenever there is occasion, ingests solid particles into the interior of its body, and there subjects them to a regular digestive process. But further, the Protophyte in its ordinary condition is motionless; and although many of the aquatic forms pass through a motile stage, this seems to have reference simply to their dispersion, and depends merely upon the rhythmical vibrations of ciliary filaments with which they are endowed in that phase of their lives: whereas the movements of the Protozoa which we have described, bear a much closer resemblance to those of the higher Animals, being executed by changes of shape in the general contractile substance of the body, and are subservient to the acquisition of food.

Thus, then, by attending to the nature of their food, the mode of its introduction, and the character of their respective movements, a line of distinction may be drawn between the Protophyte and the Protozoon, scarcely less definite than that which separates the insect from the plant whose leaves it devours, or the elephant from the tree on whose tender shoots it browses.

But although our fundamental idea of a Protozoon should be based on such examples as the preceding, yet it must be capable of extension, so as to comprehend a much wider range of forms and conditions than are displayed in the Amœba and the Actinophrys. These are, in fact, the types of a group to which the name of *Rhizopoda* was first assigned by Dujardin, and which has been proved, by recent discoveries, to have been of no mean importance in former ages of the earth's history, though now comparatively insignificant. For, as was long since asserted by Dujardin, we are not only to rank under this head the comparatively few and minute forms which inhabit fresh water, but the vast class of *Foraminifera;* whose beautifully-regular chambered shells had not unnaturally suggested the idea of their Nautiloid affinities to such as were unacquainted with the organization of their soft parts; which had been pulled-down by Ehrenberg from the rank of cuttle-fish (assigned them by D'Orbigny) to that of polypes; but which have now been finally demonstrated, by the concurrence of microscopic observations made upon the living animals, and more especially by the admirable researches of Professor Schultze,* to be true Rhizopods. They are distinguished from ordinary Rhizopods, however, by their possession of the power of forming calcareous envelopes, of which the successive segments produced by gemmation remain for the most part attached to one another, and thus give origin to shells, whose forms will vary according to the plan on which the segments increase, but are nearly always characterized by a symmetry and beauty that become most marvellous when it is remembered that they originate from minute particles of animated jelly. That the vast multiplication of the minuter forms of Foraminifera, in the seas of the Cretaceous epoch, contributed largely towards that accumulation of white mud at their

* Ueber den Organismus der Polythalamien (Foraminiferen). Leipzig, 1854.

bottom, which constitutes what we now know as Chalk, there can be no doubt whatever; although it would probably be too much to affirm (as some have done) that Chalk is entirely or even chiefly formed of their remains. But this group appears to have attained its greatest development early in the Tertiary period, to which are restricted by far the greater part of its larger forms, and in which its structural types seem the most complete and most strongly marked. The Nummulites, Orbitolites, and Orbitoides, which have now almost entirely disappeared from our ocean-waters, must then have been among the most numerous and the most widely-diffused of all forms of marine life; for a vast band of "Nummulitic limestone"—through nearly the whole of which these three types present themselves, blended in various proportions,—may be traced from the Atlantic shores of Europe and Africa, through Western Asia to Northern India, and thence to the Pacific shore of China, often 1800 miles in breadth, and frequently of from 1500 to 2000 feet in thickness; and a similar formation prevails likewise over vast areas of North America. This Nummulitic limestone does not merely *contain* Nummulites, &c., but is in general almost entirely (if not completely) made-up of them; the matrix or rock-substance in which the recognisable specimens are imbedded, being usually composed (as microscopic examination of their sections demonstrates) of the comminuted particles of similar organisms, with which smaller Foraminifera are intermingled.

Another development of the Rhizopod type seems to be presented to us in the *Polycystina;* a group of minute animals distinguished for the most part by the remarkable forms and elaborately-worked aspect of their siliceous casings. These, although occasionally met-with in the existing seas, are chiefly known to Microscopists (for whom they furnish a set of marvellously-beautiful objects) by that vast aggregation of their fossilized exuviæ, which was discovered a few years ago in Barbadoes, by Sir Robert Schomburgk. The recent observations of Professor J. Muller upon living specimens of Polycystina, seem to leave no doubt as to the close relation of the animals which form them to those of the Foraminifera.

But the most remarkable modification of this type is presented in the *Sponge*-tribe; which seems to us to have been clearly proved by recent investigations into its minute structure and developmental history, not only to be unquestionably animal in its nature, but also to form the connecting link between the Protozoa and the Polypifera. For the soft flesh with which the skeleton of the Sponge is clothed, has been found to consist of an aggregation of Amœba-like bodies; some of which are furnished with long cilia, by whose agency those currents are kept-up, which were long since observed by Dr. Grant to be continually traversing the passages and canals of the entire mass. And from the recent observations of Mr. Carter and others upon the early development of Sponges, it appears that they begin life as solitary Amœbæ; and that it is only in the midst of aggregations formed by the multiplication of these, that the characteristic sponge-structure makes its appearance. The formation of spicules is the first indication of that organization which makes the Sponge-body one whole; and these appear to originate in the calcification or silicification (as the case may be) of particular cells, or rather segments of sarcode, a distinct animal basis being found to remain when the mineral

matter of the calcareous spicules has been dissolved away by an acid. The transition between Foraminifera and Sponges is much less abrupt than at first sight appears; for among the lower forms of the first of these groups, there are some which may be described as discoidal masses of sarcode traversed by a reticulated calcareous skeleton, and only wanting a system of pores and canals to be true Sponges; whilst in certain Sponges the ordinary fibrous skeleton strengthened with spicules, is replaced by a continuous mineral reticulation. And a remarkable connecting link between the two seems to be presented in the curious *Thalassicola*, first discovered by Mr. Huxley,* and since observed by Professor Müller, which is considered by the latter as also having relations with the Polycystina. On the other hand, the passage between Sponges and the Alcyonian Zoophytes has always appeared to us to be clearly established by those intermediate forms, in which the existence of polype-mouths seems quite subordinate to that of the general spongioid body; and especially by the fact long since announced by Professor Milne-Edwards, that in the new offshoots of certain Alcyonians, the spongioid body is developed with its system of ramifying canals, before any polypes make their appearance at their orifices.

We have not yet done with this Rhizopod type of life. For there now appears to be no doubt, that we are to associate with it the curious *Gregarina*, whose place in the scale has of late been a subject of no little controversy in Germany. Considered in reference to its habitat, this creature is an Entozoon; for it is found exclusively in the intestinal canals of other animals, being almost invariably present in the Earth-worm, very common in Insects, and occurring also in Mollusks and Fishes. Each individual essentially consists of a single cell, more or less ovate in form, and sometimes considerably elongated; a sort of beak or proboscis frequently projects from one extremity; and in some instances this is furnished with a circular crown of booklets closely resembling that which is seen on the head of Tænia. The Gregarina exhibits a decided advance in grade of development as compared with Amœba; for the cell-wall is quite distinct from the cell-contents, the former being a pellucid membrane, whilst the latter consist of a milk-white fluid, usually minutely-granular, in the midst of which a pellucid nucleus is commonly to be seen. The membrane with its contents, except the nucleus, are soluble in acetic acid. This animal does not put forth digitate extensions like the Amœba, nor radiating pseudopodia like the Actinophrys; but it possesses contractility enough to be the subject of considerable changes of form, by which it executes movements of progression. In regard to its reception of food, it is conformable to the type of the Cestoid Entozoa; for these have no proper digestive cavity, and obtain their nourishment by the absorption of the juices in the midst of which they live, through the whole of their permeable surface; and it appears to be for the purpose of renewing the stratum of fluid in contact with that surface, that it is clothed with cilia. Thus, whilst not less dependent than the true Rhizopoda, upon nutrient material previously elaborated by other living organisms, the Gregarina does not perform the ingestive and digestive process which is so remarkable a feature in their life-history; this being

* Annals of Natural History, second series, vol. viii. p. 433.

rendered unnecessary in them, as it is in the Cestoid Entozoa, by the state of preparedness of the fluids they imbibe, which have been digested for them (so to speak) by the animal whose intestinal canal they infest. The multiplication of the Gregarina is sometimes effected by the simple act of self-division; but sometimes by a process which seems analogous to the formation of "zoospores" among the Protophyta. The granules dispersed within the cell aggregate into corpuscles, which, at first spherical, afterwards become boat-shaped, a large number being thus formed within each Gregarina-body. These corpuscles, at first designated "pseudo-naviculæ" from their shape, but now more commonly known as "psorosperms," are set free by the rupture of their parent-cyst; and they have been found, by the recent researches of Dr. Nathaniel Lieberkühn, to develope themselves first into Amœba-like bodies, from which Gregarinæ are subsequently evolved.* A sort of "conjugation" has been seen to take place between two individuals, whose bodies, coming in contact with each other by corresponding points, first become more globular in shape, and are then encysted by the formation of a capsule around them both; the partition-walls between their cavities disappear; and the substance of the two bodies becomes completely fused together. This conjugation, however, can scarcely be regarded as having any more significance as a true generative process, than the fusion of two or more bodies of Amœba or Actinophrys; since its products do not seem to differ in any respect from those which may be formed without conjugation in the interior of a single Gregarina-cell. Hence it seems clear that we do not yet know the whole of the life-history of this curious creature; and it is quite possible that, in common with Amœba and Actinophrys, it may give origin to some very different form.

In respect to its distinct cell-wall, and to the more complete limitation of the body which it affords, Gregarina may be considered as in some sort establishing a passage towards the group of *Infusoria* proper; the distinguishing character of which is, that the sarcode-body is included within a well-defined membrane, and that this membrane has a definite oral aperture, through which alimentary particles are introduced, with a separate anal orifice in many instances, through which the rejectamenta can be got rid of. By most of the German Microscopists, the Infusoria are considered as single cells, chiefly (as it would seem) on the ground of a "nucleus," or what appears to be such, being present in each,—notwithstanding the existence of the oral and anal openings into their interior, and the frequent presence of organs with which it is difficult to conceive of single cells being endowed. The group of Infusoria, as thus characterized (we adopt Siebold's limitation of it, excluding his order *Astoma*, of whose vegetable nature subsequent research leaves scarcely a doubt), is far less comprehensive than that of the so-called Polygastrica of Professor Ehrenberg. For it is now quite certain that among these were ranked a large number of forms belonging to the Vegetable kingdom; to which the progress of inquiry is continually adding. Thus of the vegetable nature of the group of *Desmidiaceæ* we believe that no unprejudiced observer would now entertain a doubt; for all their characters are such

* See his account of the Evolution of Gregarinæ, in Mém. de l'Acad. Roy. de Belgique, tome xvi., and subsequent notices in the Bulletin of the same Academy, tome xxi. Nos. 3 & 7.

as would lead us to associate them with Protophytes, no single attribute of animality unequivocally existing among them. And although opinions are less unanimous with respect to the *Diatomaceæ*, the preponderance is now decidedly in favour of their affinity to Desmidiaceæ. That almost everything which Professor Ehrenberg has affirmed of their organization is untrue, is the unanimous verdict of the many observers who have within recent years devoted themselves to their study. They seem, in fact, to be nothing else than isolated cells, growing and multiplying under the same conditions as those of ordinary Protophytes, and being chiefly peculiar in the consolidation of their external coat by silex. The contents of these cells have all the essential characters of a vegetable endochrome; and there is strong reason to believe that their siliceous envelope has an organic basis of cellulose. The vegetable nature of the *Volvocineæ*, advanced as probable by Siebold, has been placed beyond a question by the researches of Williamson and Busk. And to the preceding we may now add almost with certainty all the genera included by Siebold in his order Astoma, save one or two which appear to be larval forms of some higher animals. As a striking instance of the extent, to which the careful study of the life-history of the simplest Protophytes tends to modify the doctrines to which the authority of Ehrenberg has given a temporary currency, we may advert to the case of *Protococcus pluvialis;* out of the different phases of which one Plant, according to the careful observations of Dr. Cohn, Professor Ehrenberg has constructed about forty *species* belonging to fifteen *genera* of *Animalcules*.

The limits of our space forbid us from going into any details upon the varieties of form and structure presented by the true *Infusoria;* but we shall place before our readers what we believe to be the essential facts determined by recent research, with regard to their organization and life-history. The sarcode-body, which is enclosed in a distinct membrane, has usually a tolerably-definite form, and seems itself but little endowed with contractility (save in a few exceptional cases), its movements being chiefly executed by the instrumentality of its ciliary appendages. These, whether few or many, are always so disposed as not only to be subservient to the general locomotion of the body, but also to create a current towards the oral orifice. The internal substance of the body is composed of soft sarcode, in the midst of which are seen numerous "vacuoles," and also "contractile vesicles" (two to sixteen in number) which execute rhythmical movements of contraction and dilatation at tolerably regular intervals. The alimentary particles introduced through the mouth, are commonly moulded into little aggregations of a rounded form, which are received into the vacuoles; and they are often seen to execute a sort of circulation through the cavity of the body, which seems, however, to be merely maintained by the successive introduction of new alimentary particles, each aggregation of which pushes-on its predecessors. Thus the pellets that first entered, gradually make their way to the anal orifice, yielding-up in their course their nutritive materials; or, if no such orifice exist, they either find their way back to the mouth, or (it is believed) force their way out by an extempore anus through the limitary membrane. The multiplication of Infusoria by the process of duplicative self-division, is a process that has long been familiar to Microscopists; but various other

modes of propagation have become known of late years. Thus it has been shown by Stein, Jules Haime, and others, that many Infusoria at certain times undergo an *encysting* process; the phenomena of which were completely misapprehended by Professor Ehrenberg. The Animalcule loses its activity, its form becomes more rounded, and its cilia or other filamentous prolongations are either lost or retracted. The body then secretes from its surface a sort of gelatinous case, which hardens so as completely to enclose it; the Animalcule, however, still remaining free in the midst of its coffin-like investment. This condition was not unknown to Professor Ehrenberg, who considered the encysting process as the expiring effort of life; but if the cysts and their contents be attentively watched, it will be seen to be preliminary to the production of new individuals. This production may take place in different modes. For sometimes the substance of the body appears to break up into numerous "gemmules," analogous to the "psorosperms" of Gregarina, and to the "zoospores" of Protophytes; and these, when set free by the bursting of the cyst, swim forth to develope themselves into a new brood of Animalcules of the same type with that from which they sprang, though at first perhaps bearing little resemblance to it. But in other instances, only a single offspring is developed from the nucleus of the original cell-body; which offspring may have a very dissimilar form. Thus the *Vorticella* gives origin, through this encysting process, to an *Acineta,* which is very like an Actinophrys; and this Acineta, acquiring a stalk but still retaining its general characters, assumes the form which has been distinguished as *Podophrya.* From the nucleus of this is evolved an internal bud, which gradually comes to present the form of a young Vorticella; and this, escaping from the Acineta-body by a gap formed in some part of its wall, goes forth to originate a new colony of Vorticellæ; whilst the Acineta, the gap in whose wall soon closes-up again, goes-on stretching out and retracting its radiating filaments, and after a time produces in its interior a new nucleus for a second Vorticella bud.

Neither of these processes, however, can be fairly looked-on in any other light, than as modifications of the general plan of multiplication by gemmation, which corresponds, in its essential features, with the *growth* of the higher animals. With anything that can be truly accounted their *generation,* we are yet entirely unacquainted; and it is, therefore, quite possible, that the complete life-history of Infusoria may include some phases of which we have not at present any idea. By Professor Agassiz, indeed, it has been asserted that *Paramecium* and *Bursaria,* two genera which are usually considered as among the most typical of Infusoria, are nothing else than the larvæ of Planaria; and if this were proved in regard to them, we should be disposed to regard the entire class as merely consisting of embryonic forms of higher animals. But we cannot help believing that Professor Agassiz has been misled on this point by imperfect observation; more especially since, as we have lately learned from Dr. Wagener, the Cercaria-like embryos which come forth from the ova of some Trematode worms, although so like Infusoria in their general aspect as to be readily mistaken for them, differ from them in this essential particular,—that they possess the water-vascular system characteristic of the adults, in the same rudimentary form in which it presents itself among the Rotifera.

Having thus endeavoured to place our readers *au courant* with the general state of knowledge on this subject,—which we have entered into thus fully, because it involves considerations of the highest interest and importance, alike in Physiology and in Zoology,—we have to inquire how far the mode in which it is treated in our two recent British treatises on Comparative Anatomy can be regarded as satisfactory.

Ignoring altogether the term "Protozoa," which seems to us singularly appropriate, Professor Owen ranks as a sub-province of Cuvier's Radiata, under the common designation "Infusoria," the *Rotifera*, the *Rhizopoda*, and the *Polygastria;* the Sponges being altogether left out. Now the Rotifera, since their complex organization was first (however imperfectly) made known by Professor Ehrenberg, have been ranked by all who have attentively studied them, in a far higher part of the animal series, namely, in some part of the Articulated sub-kingdom. Thus Professor Leydig, who has published a most important monograph upon this group, considers it most allied to the Crustacea, and designates it *Cilio-Crustacea.* By Mr. Huxley, again, the resemblance of certain Wheel-Animalcules to the larval forms of certain Marine Worms, and the presence of a water-vascular system in the one group as in the other, is considered (and we think with justice) as indicating that the special affinity of the Rotifera is with the Annelida. And in a recent communication to the Royal Society, Mr. Gosse has given strong confirmation to the doctrine of their Articulated nature, by showing that the parts of their curious masticating apparatus may be fairly considered as homologous with the buccal apparatus of Mandibulate Insects. We looked with some interest, therefore, to Professor Owen's account of this group; expecting to find him assigning some reasons for still keeping it under the Radiated sub-kingdom, and for degrading it to the lowest of the provinces into which he divides this; but have found none whatever. For anything that he tells us, the student would be left in utter ignorance of the general doctrine of the best informed Naturalists on this point, and would be not a little surprised and perplexed at finding the Rotifera so differently placed in almost every other modern treatise on Invertebrate Anatomy or Zoology.

The retention of the term Polygastria, as the designation of the group to which Siebold and those who follow him limit the term Infusoria, seems to us extremely undesirable, as tending to keep before the mind the *polygastric* hypothesis of Professor Ehrenberg, which has now been fully proved to have been founded upon an entirely erroneous conception of the real nature of these animalcules. Even Professor Owen speaks of this hypothesis as borne down by the weight of opposing evidence; yet he repeats some of Professor Ehrenberg's descriptions, and allows his large figures of the "Monad of Volvox," "Vorticella," and "Leucophrys," still to stare his readers in the face, as if for the very purpose of impressing his erroneous views on their minds. Further, although he adopts the term Rhizopoda, he makes no distinct separation between them and the Polygastria; the Foraminifera are not so much as mentioned; while Sponges are left, together with a number of undoubted Plants, in that limbo between the Animal and Vegetable kingdoms, "in which the character of the organized fundamental nucleated cell is retained, with comparatively little change or superaddition;"—all those recent additions to

our knowledge of them which seem to have conclusively established their title to rank as Animals, being entirely ignored.

But this is by no means all. Professor Owen has attended so little to the recent progress of inquiry upon the border-groups of the Animal and Vegetable kingdoms, that he seems utterly unaware that the place of many creatures which he continues to describe and delineate among Animalcules, has been definitely shown to be on the Vegetable side of the boundary-line; and his chapter on the Polygastria is consequently made-up of a most heterogeneous collection of "facts and figures," in which Diatomaceæ and Desmidiaceæ, Volvocineæ and Palmelleæ, are made to do duty as Animalcules, in opposition to the conclusions of the most competent among the recent investigators into their nature and history. And it is from not having applied himself to the impartial consideration of the evidence, that he raises objections to the physiological distinction which has been drawn between the two kingdoms—as we believe, upon the most satisfactory grounds. For he deems it a sufficient invalidation of the doctrine that true Plants make their own organic compounds, whilst true Animals derive theirs from bodies previously organized, to say that masses of Animalcules have been known to decompose carbonic acid and to give off oxygen, like Plants, under the influence of sun light: the fact being, that the supposed Animalcules (*Frustuliæ, Chlamydomonas, Euglena*) are the very creatures which have now been proved by *other evidence* to be really Plants; so that here, as in many other cases, *exceptio probat regulam.* —Let us compare this with another somewhat analogous instance.

Suppose that the distinguished Professor by whom the importance of the characters furnished by the minute structure of the *teeth* was first demonstrated, had been led to distrust the value of any deduction respecting the nature of a *bone* that might be drawn from its microscopic appearances, because a bone which he believed to be that of a Bird was pronounced on that authority to be that of a Reptile,—and suppose that this very bone was afterwards proved, even to the satisfaction of the Professor himself, to be reptilian,—would not the value of the microscopic test, instead of being invalidated by the supposed disproof of its reliability, be immensely raised by this evidence of its essential superiority to characters furnished by imperfectly-preserved external configuration? This, as Professor Owen well knows, is no hypothetical case; and its parallelism is obvious.

Until reliable evidence shall be offered to the contrary, therefore, we think it may be held as certain, that the peculiar attribute of the higher Plants is equally characteristic of the lower; and that whenever any aquatic organism is found to decompose carbonic acid under the influence of sun light, and to set free oxygen, that organism may be ranked as a vegetable, however active may be its movements. It may be said that this is "begging the question;" but we reply that in all the cases hitherto cited of this kind, the proof of the vegetable nature of these organisms has been drawn from independent sources. We may mention the following case in illustration, as having occurred to ourselves about ten years since. The water of a rain-water cistern, which had been thoroughly cleaned out, and which had been filled a few days afterwards by a heavy thunder-shower, was observed to present a greenish tinge; and it was

noticed that a green froth, full of minute bubbles, came to the surface whenever the sun shone on it. On examining a portion of this froth under the microscope, we found that the water was crowded with green cells in active motion; and although the only bodies at all resembling them of which we could find any description, were the so-called Animalcules constituting the genus *Chlamydomonas* of Ehrenberg, and very little was known at that time of the motile conditions of Plants of this description, yet of the vegetable nature of these organisms we could not entertain the smallest doubt. For in all their essential microscopic characters, and in their mode of multiplication, they corresponded with undoubted Protophytes; they appeared in freshly-collected rain-water, and could not, therefore, be deriving their support from organic matter; and under the influence of light they were obviously decomposing carbonic acid and setting free oxygen. Our attention was soon attracted from these little beings to an enormous swarm of Wheel-Animalcules, which soon made their appearance, and which greedily devoured their predecessors. But had we followed out their complete history, as Dr. Cohn has since done, we should have found that our Chlamydomonas was nothing else than the *motile* form of *Protococcus pluvialis*, that alternates under certain conditions with a *still* form, in which its Vegetable nature is manifested beyond a doubt.

In his general treatment of this part of his subject, Professor Rymer Jones seems to us to have a much truer appreciation of the present aspect of our knowledge of it, than is displayed by Professor Owen; but when we come to particulars, we find some very unaccountable blemishes. He adopts the designation Protozoa, and gives to the group precisely the same range as we should ourselves assign to it. But strange to say, the first order of beings described by him under this head, is that of *Spermatozoa;* which, as the history of their development and actions fully demonstrates, have no more title to be regarded as independent organisms than have blood-corpuscles or ciliated epithelium-cells. From these he proceeds to an account of Amœba and Actinophrys, and thence to the Rhizopods, Foraminifera, and Sponges; as to his description of all of which we have only to speak in terms of commendation. In his description of the so-called polygastric Infusoria, he returns to the views which he had the merit of being one of the first to promulgate, in opposition to the authority of Professor Ehrenberg, as to the non-existence of an alimentary canal and multiple pedunculated stomachs; having, in the interval between his first and second editions, avowed his conversion to the polygastric doctrine.* The general account of the group remains essentially the same as in the previous edition, the chief additions being derived from the treatise of M. Dujardin, with just enough reference to Siebold to show that he was acquainted with his views. Of everything that has been done by Stein, Cohn, Haime, and others, during the last few years, he seems to be in profound ignorance.

The next of Professor Owen's "sub-provinces" is that of *Entozoa;* which he divides, as in his former writings on that subject, into Cœlelmintha and Sterelmintha; nominally adding thereto the division Turbellaria (founded by Ehrenberg, and more precisely and completely established

* See Cyclopædia of Anatomy, vol. iv. p. 14.

by Oersted and Schultze), but in his more detailed account of it continuing to rank it with the Trematode Worms. To continue to keep the Intestinal Worms out of the Articulated sub-kingdom, with many undoubted members of which they have the closest affinity, seems to us to imply an unaccountable want of appreciation of the essential features of their organisation; and as all the most eminent Continental Naturalists agree, we believe, with Siebold and Quatrefages, in ranking the *Helminthes* (Entozoa), *Turbellaria*, *Rotifera*, and *Annelida*, as the Vermiform subdivision of the Articulate series, we should have been glad to learn Professor Owen's reasons for leaving them where he does. We can find no other than the filamentous character of the nervous system; a part of the organism whose completely subordinate rank in these creatures, entirely forbids, as it seems to us, any especial value being attached to such a condition as a basis of classification. Both our authors, in treating of this group, notice some of the important Continental researches, by which it has been conclusively proved that the so-called Cystic Entozoa are nothing else than abnormally-developed Cestoid Worms; but neither of them seems to be at all acquainted with the completeness which has been given to these researches during the last four or five years; for none of the more recent memoirs on this point are cited (even the important work of Van Beneden, reviewed in our tenth volume, being altogether unnoticed by Professor Owen), and the Cestoidea are still ranked in each work as a separate group, although we are told in both that they are all *probably* tænioid larvæ. We are sorry to be obliged to add, moreover, that both authors continue to repeat the erroneous statement, that the longitudinal canals of the Tænia and other Cestoid worms, represent a digestive apparatus; the fact having now been most fully substantiated (especially in the recent beautiful monograph of Dr. Guido Wagener), that these canals constitute a "water-vascular" system, as was affirmed by Siebold in 1850. And we must here add, that this system, whose proper interpretation by Siebold constitutes one of the most remarkable of all modern advances in invertebrate anatomy, and whose true import and relations are an object of special attention with every real student of Helminthology, is scarcely mentioned by name in either of our British systematic treatises.

The sub-province *Radiaria* seems intended by Professor Owen to include those of Cuvier's Radiata, in which radial symmetry is a predominating characteristic; since he associates in it the Echinodermata, Acalephæ, and true Zoophytes, still retaining in immediate connexion with the latter the Bryozoa (more properly Polyzoa), notwithstanding what he himself admits to be their strong Molluscan affinities. We are sorry to be again called-upon to remark upon certain notable omissions and errors, which detract very much from the general excellence of Professor Owen's two lectures on Polypi. Thus in treating of the sexual generation of the Compound Hydrozoa, a subject which is rendered difficult by the apparent variety of the organs by which the function is accomplished, he omits to notice Professor Allman's valuable memoir on *Cordylophora*,* in which the essential conformity existing amidst all these varieties is pointed-out, in accordance with the interpretation suggested by one of the most

* Philosophical Transactions, 1853.

remarkable of their intermediate forms. Whether the reason of this silence lies in the fact, that Professor Allman takes occasion to state his accordance with us (vols. i. and iv.) in our interpretation of the so-called "Alternation of Generations," and thus implies his dissent from Professor Owen's hypothesis of "Parthenogenesis," we can of course only surmise. Again, in describing the anatomy of Actinia, he repeats the now antiquated error, that the convoluted tubes which lie in the chambers that surround the stomach are testes, and that these animals are consequently androgynous; the fact having been clearly proved, that these tubes contain "thread-cells," or "filiferous capsules," exactly resembling those of the integument, and that the so-called ovaria contain sperm-cells and spermatozoa in some individuals, and ova in others, the sexes being separate, as was shown fifteen years since by the independent researches of Kölliker and Erdl.—Like errors of omission and commission are found in Professor Rymer Jones's treatment of this part of the subject; and he seems to have made no attempt to extricate himself from that strange confusion between the different subdivisions of the Anthozoa, arising from his adherence to a principle of classification now entirely exploded, under which he laboured at the period of his first edition, and which his article Polypifera, in the 'Cyclopædia of Anatomy and Physiology,' showed to have been not cleared-up by the detailed study of the group. He separates the Bryozoa from the true Zoophytes by the interposition of the Entozoa; but he says not a word of their Molluscan relations; indeed, by ranking them between Entozoa and Rotifera, he would seem rather to regard them as having vermiform affinities. Professor Owen, however, seems fully sensible of the close approximation made by the Bryozoa to the Compound Ascidians which form the lowest step in the Molluscous series; and he justifies his wide separation of the two on the ground of what he affirms to be an essential difference in their developmental history:—"No compound Ascidian," he remarks, "quits the ovum as a gemmule swimming by means of cilia either generally diffused, or aggregated on special lobes after the type of the Rotifer," as is the case with the embryos of certain Bryozoa; "and no Bryozoon quits the ovum in the guise of a Cercarian, to swim abroad by the alternate inflections of a caudal appendage." But does he forget that such an embryonic condition as that of the Bryozoa is characteristic of some of the most typical Mollusca; whilst the tadpole-like embryo of the Ascidians is unlike every other Molluscan embryo, its peculiar endowments being limited to that one group, and being consequently a special Ascidian, and not a general Molluscan character?

Let us see how this case stands. That Bryozoa should have been formerly ranked as Zoophytes, is not surprising, when we consider their similarity to that group in habit of life and in general aspect; but in proportion as their true structure has been disclosed by microscopic research, have their points of difference become more and more apparent, and their points of approximation to Mollusca (first pointed out by Milne-Edwards and Audouin) more clearly discernible. Thus, in the first place, all true polypes use their tentacula to grasp their food and convey it to the mouth; and if their surfaces possess cilia, these take no share in the ingestion of aliment. On the other hand, in Bryozoa, as in all Acephalous Mollusca, the nutritive particles are drawn in by a ciliary current, which also serves

to aërate the fluids. In no true polype is there a separate intestine and anal orifice, nor does the digestive apparatus hang freely in the visceral cavity; in the Bryozoa, as in the Mollusca, we find both these characters of elevation, together with (in certain species) a gizzard-like organ, and a cluster of biliary follicles in and around the stomach, closely resembling those of the Compound Tunicata. The relative position of the oral and anal orifices, again, and the position of the single nervous ganglion between them, are essentially Molluscan characters. The absence of a heart and circulating system in Bryozoa is, it is true, a character of degradation; but this apparatus is already so extremely degraded in the Tunicated Mollusks, as to be only removed by one step from that provision for the movement of fluid in the general cavity of the body, which represents in Bryozoa the blood-circulation of higher animals. The propagation by gemmation, although formerly supposed to be a character exclusively Zoophytic, is common also to the greater part of the Tunicata. And although many of the composite fabrics of Bryozoa have a stony density, and closely resemble the solid polypidoms of certain Anthozoa, yet in others, especially among the fresh-water species, we find a very close resemblance to the gelatinous bed or leathery crust in which the Compound Ascidians are lodged. To us, therefore, it seems clear that the Bryozoa and Tunicata ought to be placed in close approximation to each other, their general plan of conformation being no more different than that of any other two classes of the Molluscous series; and that, taking the same rank in that series with the Vermiform group among the Articulata, they present various relations of *analogy* to members of that group, though none of *affinity*. The term *Molluscoida* has been proposed by Milne-Edwards as a distinctive designation for this group; but such a separation seems scarcely required by any essential difference in *plan* from that of the true Mollusca, to whose "archetype" it has been shown to approach very closely,* the chief differences lying in grade of development. Professor Owen again adverts to this question, when treating of the Tunicata in a later part of the volume; and the following sentence will, we think, afford pretty satisfactory evidence, in the obscurity of its ideas and the involution of its style, that he has by no means "thought himself clear" upon the subject:

"If these significant indications of the fundamental affinity of the Polypes, with the retention of the polype form, and the absence of a respiratory organ in the highest of the class should beget a doubt as to the propriety of calling a Bryozoon a Mollusk, and thereby losing the advantage of the latter term as a definite and intelligible sign of a certain advance of organization, the comparative anatomist, whilst admitting the full amount of the affinity of the Bryozoa with the Tunicata, and thereby illustrating his view of the Molluscous series as constituting a great parallel branch of the Animal kingdom with the articulate series, may anticipate a verdict in favour of his judgment, in the necessarily artificial mode of successively treating of the different types and grades of organization, if he should select the compound Ascidians as the point at which, for his needs of description and generalization, he severs an unequivocally natural series of animals from the wide-spread root or base from which it springs." (p. 473.)

If our readers comprehend this, it is more than we can do.

* See the article Mollusca in the English Cyclopædia, vol. iii.

At the commencement of Professor Owen's ninth Lecture, On the *Acalephæ*, we are startled by the following statement:

"In the preceding lecture we saw that, whilst the new individuals propagated by gemmation were for the most part like the parent, those that came from the ova were in very few instances like the parent, but underwent a considerable metamorphosis. They quitted the egg-state either as a ciliated planula under the guise of a leucophrys, or were partially ciliated on special lobes, like a rotifer; or, what was more extraordinary, they came forth under the form of an animal which is usually ranked as a member of the higher class of Radiata, viz., a free-swimming, bell-shaped, or discoid medusa." (p. 157.)

On turning to the preceding lecture for the justification of this last statement, which seems to us to manifest a complete want of comprehension of the facts of the case, we find it stated (p. 155) that in the marine *Hydrozoa*—

"The offspring developed in the ovi-capsules are, as a general rule, the ciliated larvæ called 'planulæ;' the *Plumularia coronata* offering an exception analogous to the *Alcyonella* in the highest, and the *Hydra* in the lowest, class of polypes; whilst other *Plumulariæ*, the *Corynidæ*, and certain species of *Campanularia*, deviate in a still more remarkable manner by the development and liberation of the locomotive offspring in the guise of a minute *Medusa*."

Still more astonished at the assertion that a Zoophyte ever produces a medusan *embryo*,—the fact having been established beyond all question that the Medusa is a *bud* from the Zoophyte, containing its sexual apparatus,—we turn back to the lecture on Hydrozoa, where we find it stated correctly enough (p. 131) that the Coryne originally developes a many-armed nutritive polype or individual; but that a set of *buds* developed around the base of the first polype, instead of repeating the form and condition of that animal, take-on a higher form, resembling that of a bell-shaped Medusa, become detached, and swim off to a distance, forming and discharging the ova, which in their turn develope the fixed polype-shaped Coryne. This history, in its essential features, is true of all the Compound Hydroida and of the Medusan Acalephæ; which thus are but two states, or rather parts, of one and the same kind of organisms. As we long ago maintained, in opposition to the "Parthenogenesis" doctrine of Professor Owen, the Medusan buds are the sexual organs of the Hydroid Zoophyte; and neither can be regarded in itself as a complete organism, any more than a Plant can be said to be a complete organism without its floral apparatus, or its floral apparatus without its stem and leaves. Whilst, in the ordinary *Corynidæ*, the Medusa-buds detach themselves, and swim freely away, before maturing their ova or spermatozoa, in the fresh-water *Cordylophora*, the generative bud does not assume the characteristic Medusan form, although presenting (as Professor Allman has shown) the essential features of the Medusan structure, and developes its ova or its spermatozoa whilst still in connexion with its stock; and its ova, when fertilized by the spermatozoa, evolve themselves first into the form of ciliated gemmules, and then into that of Hydroid polypes. So among the *Campanularidæ*, in which the generative buds (like the ordinary polypes) are produced in clusters within horny capsules, these buds evolve themselves in some species into the form of independent Medusæ; whilst in others they do not detach themselves, but expand one after another into the Medusan form at the mouth of the capsule, withering

and dropping-off after they have matured their generative products; and in other cases, again, in which the medusan conformation of the sexual gemmæ is obscured by want of development, the generative act is performed whilst they are still enclosed within their capsules. This last is the only mode of generation that has yet been witnessed among the *Sertularidæ;* for no free Medusoids have been observed to make their way out of the (so called) ovigerous capsules of this family, the bodies developed within which, although commonly reputed to be eggs, are really sexual gemmæ, containing sperm-cells or ova as the case may be, though never attaining the condition of Medusæ. It is a complete misconception to affirm, as Professor Owen does (p. 161), that "the bell-shaped medusoid which Dalyell saw struggling to escape from the ovi-capsule of the *Campanularia,* is the equivalent, or homologue, of the ciliated planula, which in like manner escapes from the ovi-capsule of the *Sertularia;*" for the homologue of the "bell-shaped medusoid" is the ovigerous or spermigerous gemma, which, in the Sertularidæ as in some Campanularidæ, remains within the capsule, unexpanded into a Medusoid; while the equivalent of the "ciliated planula" is the gemmule, which, in the Campanularidæ as in the Sertularidæ, is the first product of the true generative operation, whether this be performed by free medusoids or by gemmæ whose development into the medusan form has been arrested.

Now among the Hydroida, the zoophytic form is that which attracts most attention, and by which, therefore, the organisms belonging to it have hitherto always been designated; the fact that the true generative apparatus of many of them was developed in the form of free-swimming medusans, having until lately escaped observation. On the other hand, the Pulmograde Acalephæ have until recently been known only in the Medusan stage of their existence; their origination as gemmæ from Hydroid Polypes having likewise been a discovery of the present era. But now that the absolute identity of the two processes has been substantiated, can any sufficient ground be assigned for keeping apart the two groups of which they are severally characteristic? The analogy of the Vegetable kingdom will supply us with a useful basis of comparison. In many Phanerogamia, especially the groups that furnish our most valuable timber-trees, the vegetative portion of the organism,—namely, the stem and roots, the branches and leaves,—is so predominant, and the generative apparatus is so imperfectly developed, that our "idea" of an oak, an elm, a beech or a fir, is almost entirely based on the characteristic aspect of their aggregate. But there are other Plants of which the flower or generative apparatus is the only ostensible part, the vegetative being altogether subordinate, and perhaps so completely concealed as to attract no attention; thus, for example, many persons only know the Colchicum by its autumnal blossom, and are not at all aware that it sends-up a leaf-stalk in early spring, which dies-down some months before the flower appears; the remarkable parasitic Rafflesia seems to be "all flower," its nutriment being drawn already elaborated from the plant upon which it sprouts; whilst, again, the *Vallisneria spiralis* only makes its existence known to us in its native streams, by sending its unisexual flowers to their surface, of which the female still remains in connexion with the plant at the bottom through the intermediation of an elastic

spiral stem, but the male detaches itself altogether whilst still quite immature, floats to the surface, expands there, and performs the act of fecundation long after its separation. Now would any one dream of classifying Plants according to whether their vegetative or their reproductive apparatus, their foliage or their flowers, happened to make the strongest impression upon our senses; or to separate the Colchicum and the Vallisneria from other plants, because we may never happen to see any part of them but their blossoms? The *scientific* "idea" of a Plant involves the entire organism, its apparatus of nutrition with its apparatus of generation; and this is common to all the cases we have cited, notwithstanding the extreme difference which is presented by different tribes in the relative proportions which these two apparatuses bear to one another. And just on the same principle, the scientific idea of a Hydroid Zoophyte ought to be made to include its medusoid as well as its polypoid buds, notwithstanding that the former may be so imperfectly developed as to constitute no obvious feature in their organisation; whilst the scientific idea of a Medusa must include the antecedent polype-stock from which it has been budded-off. Professor Owen, however, justifies his retention of the class by an analogy of a very different kind, that of those Insects which pass the greater part of their lives under ground or in water in the larval state (in which they are on a level with the vermiform articulata), and which only present themselves for a brief period in the perfect or imago state; for he remarks, "we do not class the cockchafer and the May-fly with the Vermes, as we ought to do according to the analogy of the Campanularia and the Coryne." The two cases, however, are far from having the parallelism which he assigns to them. The larval insect is *not* a worm; for however much it may resemble a worm in its general grade of organisation, it has no generative apparatus, and is therefore not a complete animal, to be classed among the Vermes: whilst, again, the whole of its progress towards the higher form is marked by the progressive development of parts which are added or substituted, for the completion of the organism; and it is quite as just, therefore, to take this perfect form as the type of its class, as to base the typical characters of the human species rather upon the entirely-developed organism, than upon any of the earlier phases of it. But the Medusa does not stand to the Hydroid Zoophyte in this relation; for the former cannot be regarded as the perfected type of the latter, any more than the latter can be regarded as a complete organism without the former. The polype-stock alone is like a worm without sexual organs; the medusa alone is like the sexual apparatus detached (as certain worms do detach it) from the body that developed it.

The view which we advocate is supported by all those recent additions to our knowledge of the *Cirrhigrade* and *Physograde* Acalephæ, of which the recent researches of Huxley, Kölliker, Vogt, Leuckart, and other eminent Anatomists have been so productive. Here again we have to notice the unaccountable omission, on the part of both our Authors, of all reference to these masterly investigations, which have contributed to a very general accordance among all those who have studied this curious group of animals, as to their real nature and relations, which are concisely expressed by Kölliker's term "Schwimmpolypen." For the *Velella* and

Physalia, the *Diphyes* and the *Siphonophora*, whose nature has been a source of perplexity to all Zoologists who have sought to understand them as *simple* animals, are readily comprehended when their general plan is compared with that of any one of the composite Hydroida, and allowance is made for the speciality of organisation by which it is adapted for free locomotion. Thus, the well-known *Physalia*, or "Portuguese man-of-war," has been anatomised as if its great air-vesicle or float were the essential part of its body; and all sorts of speculations have been put forth with regard to its nature, the cirrhi dependent from its under side being looked-upon as quite subordinate organs. But of these cirrhi it is now known that the shorter ones, like those of the little Velella, are so many hydroid polypes, in mutual communication with each other, so as to form a polygastric digestive apparatus, to which the air-vesicle of the Physalia stands in the same relation, as the delicate horizontal plate of the Velella, with its vertical crest or sail, does to the polypoid cirrhi dependent from its under side; whilst, as these polype-mouths are not themselves furnished with tentacula, additional appendages of this kind are developed for supplying them with food. In each case, the generative function is provided-for by the development of medusoid-buds, which (as in the ordinary Hydroida) sometimes become detached, sometimes remain in continuity with the stock, evolving spermatozoa or ova; and the generative product appears to be a polypoid animal, from which the entire composite body is gradually evolved by gemmation.—We cannot but think it singular that Professor Owen should not have been aware, that Mr. Huxley communicated to the Linnæan Society, as long ago as 1849, a memoir on these Composite Acalephæ, containing the results of observations which he had made upon them during his four years' voyage as assistant-surgeon in the surveying-ship *Rattlesnake;* that the funds of that Society not enabling it to publish Mr. Huxley's memoir, he applied to the Government for the necessary means, and was kept by it in a state of suspense for several years; and that at last, the Government Grant Committee of the Royal Society appropriated a large sum to this purpose, so that the publication of Mr. Huxley's researches (which has been in great part anticipated by that of the observations of Kölliker, Leuckart, and Vogt, though these were not made until after Mr. Huxley's memoir had been communicated to the Linnæan Society) may now be speedily looked-for. And yet, as if he were perfectly unaware that either Mr. Huxley or any one else had already attained the solution of the complicated problem which this group of animals presents, he concludes his Lecture on the Acalephæ with this passage:

> "With regard to the development of the ciliograde and physograde species, scarcely anything connected or precise is at present known. The medical officer who may be destined for foreign service, and to whom the study of Nature offers any charm, could hardly contribute observations more valuable to natural history, than such as he might be able to make on the generation and development of the Pelagic Acalephæ."

Here, again, we are tempted to inquire whether Mr. Huxley's very pointed repudiation of the whole doctrine of Alternation of Generations, and of Professor Owen's parthenogenetic modification of it, can have anything to do with the Hunterian Professor's very marked abstinence

from all allusion to his labours on this subject. That he should take no notice of what Kölliker, Vogt, and Leuckart have published, is certainly surprising, but scarcely *so* surprising.

Although Professor Rymer Jones designates the Acalephæ of Cuvier by the term Hydrozoa, yet he appears to have made this change solely on the ground of the polypoid origin of the Medusan forms of the class; and his notice of the Physograda, Cirrhigrada, and Diphyda, the very forms which recent researches have shown to preserve the polypoid character through the whole of life, stands almost exactly as it did in the first edition, the only addition made to it being one that is by no means accordant with the existing state of our knowledge.

Both authors, as might be expected, give a tolerably-full account, with illustrative figures, of the developmental history of the *Cyanæa aurita*, as established by the observations of Sars, Siebold, Dalyell, and others; but neither makes any mention of the fact, which we hold to be of fundamental importance in the interpretation of the process, that the original polype-stock (the *Hydra tuba* of Dalyell), instead of *dividing* itself into medusa-disks, buds-off a pile of medusa-disks, and may even recommence its polypoid mode of gemmation after doing so. It is this fact, which, as we have shown on a former occasion,* establishes the essential homology between the polype-stock of the Campanularia and that of the Cyanæa; the difference between the two lying only in the fact, that the polype-buds of the former remain in continuity with each other, so as to form that composite structure by which the species is best known; whilst those of the latter detach themselves when mature from the parent-stock (like those of the common Hydra), and their medusa-buds, instead of being so minute as to escape notice, unless searched for by a Microscopist, evolve themselves into those massive forms which force themselves upon the attention of every observer. The life-history of the one organism, however, is so completely the same in all its essential particulars, with that of the other, that it is difficult to see on what ground the two could be ranked in distinct classes, if we were now to become acquainted with them for the first time, instead of having our view of them prejudiced by long usage.

Professor Owen's account of the class *Echinodermata*, although not in every respect what could be wished, is on the whole satisfactory. He gives a pretty full account of Professor Müller's recent researches on the curious larval states of this group; but this account is not furnished with the illustrations requisite to enable the descriptions to be comprehended. We will defy any one to form the least idea of their marvellous shapes without the aid of figures; and even the best delineations can give but a very imperfect notion of them. The chief point on which Professor Owen's account of the developmental history of these Echinoderms requires amendment, is his account, on the authority of Sars, of the development of *Echinaster sanguinolentus*. This history is so inconsistent with that of the development of the Star-fish larvæ afterwards observed by Müller, that it obviously needed revision; and this revision was made by Busch (a pupil of Professor Müller) in 1851. He found that the body regarded by Sars as a mere pedicle or organ of attachment for the

* Vol. i. p. 163.

young Star-fish, is really a larva-zooid, having a stomach and probably a mouth of its own, and is thus analogous to the larvæ of other Star-fishes, though of smaller relative size and less complex structure; the difference being apparently related to the peculiar circumstances under which the development takes-place in this type, the larva being retained within a sort of marsupial chamber, formed by the drawing-together of the rays of the parent around its mouth, instead of being sent to sea to take care of itself, as is the case with the Star-fish larvæ generally.—The want of illustrations cannot be charged against Professor Rymer Jones's Chapter on this subject; for in addition to the copious and beautifully-executed figures which illustrated this part of the previous edition, numerous wood-engravings of the same high class, copied from the admirable representations of Professor Müller, have been introduced in elucidation of the descriptions. We cannot, however, speak in terms of commendation of the chapter as a whole; since it contains many glaring errors. The mode, for example, in which Professor Rymer Jones would turn an Alcyonian Polype into an Encrinite (p. 203), shows an ignorance or forgetfulness of some of the most essential features of the structure of the latter; and the assertion (p. 204) that the *Comatula* may be considered "one of the lowest of the Asteroid Echinodermata," must excite a lively emotion of wonder in the mind of every one who is acquainted with its true relations. For, as was long since pointed-out by Professor E. Forbes, the Comatula is essentially *a free Crinoid;* the earlier part of its life being passed in the true crinoidal state; and its entire organization being conformable to that type. One of the most striking features of that type is the presence of an intestinal tube and a distinct anal orifice, which places the Comatula as much above all ordinary Star-fish in this respect, as it is in the activity of its locomotion. Professor Rymer Jones does not give the slightest indication of being acquainted with Mr. J. V. Thomson's twenty-years'-old discovery, that the little body which he had previously considered as a Pentacrinus, is really the larval condition of the Comatula; and like Professor Owen, Professor Rymer Jones repeats Sars's account of the development of Echinaster, without the correction which alters the entire interpretation of the facts accurately recorded by Sars.

We have now arrived at the conclusion of the task which we have imposed on ourselves; and we lay down our pen with the feeling of great regret, that we have been obliged to execute it in a spirit of such constant depreciation. That much labour and ability have been brought to bear by both our Authors in the preparation of these new editions, none can be more fully aware than those who, like ourselves, have had occasion to go over the very same ground. And we can only lament that the want of determination to bring their works *thoroughly* up to the present state of knowledge, has led their Authors too often to rest satisfied with additions, when alterations, suppressions, and entire recastings were quite as much needed. We could easily show, if it were necessary, that this statement is no less true of the portions of both volumes which we have left unnoticed, than it is of those to the critical examination of which the preceding pages have been devoted. And we could also point, in the notes to Professor Owen's lectures, to many allusions to the writings of

contemporary authors, that betray a disposition to exalt himself at their expense, which will assuredly not increase the estimation in which he is held. In more than one instance, moreover, he makes these allusions to past editions of their writings, and cavils at statements which he can scarcely help knowing them to have since qualified or withdrawn. Would Professor Owen like some of his own earlier memoirs to be quoted against himself? Would he take his stand on his first 'Anatomy of the Terebratula,' or on the ornithic character of the wing-bones from the Maidstone chalk?

We cannot charge Professor Owen with any neglect of his predecessors in the way of citation; and the very copious list of works referred to, which concludes his volume, not only bears testimony to the extent of his research, but will be very useful as a guide to those who desire to ascertain the authority for his descriptions. The example of one who has confessedly the greatest opportunities of personal study of Comparative Anatomy at his command, and who never omits to draw the attention of his readers to any point on which he thinks he may lay claim to originality, should be a sufficient assurance that the materials of any such comprehensive systematic treatise *must* be largely drawn from the labours of others; and should make critics hesitate in stigmatizing as mere compilers, writers who honestly avow this necessity. We feel called upon also to remark, that Professor Owen seldom gives the least hint of the source of his illustrations (the number of which in this edition is not increased in by any means due proportion to the text), so that for anything that appears to the contrary, the reader might suppose them to be original. So far is this from being the case, however, that the greater number of them are borrowed without the least acknowledgment; a proceeding of which we cannot see any justification, and which appears to us manifestly unfair towards the original delineators. With the Hunterian Museum at his command, we should have thought that Professor Owen would have considered it more creditable to avail himself of the ample store of subjects it contains for the draughtsman's pencil, than to have recourse to a wholesale appropriation of the delineations of others, which can only be thought excusable when fully acknowledged.

Professor Rymer Jones contents himself with a few references at the foot of his pages; and of these not a few are worthless, his authorities having been often superseded by others more modern and more trustworthy. No fewer than sixty-two new illustrations have been introduced; and these are of the same high character with those which constituted so remarkable a feature of the previous edition. We find ourselves obliged to repeat our animadversion, however, with respect to the unacknowledged appropriation of the delineations of others; since very few, if any, of Professor Rymer Jones's illustrations are original, and their sources are very seldom indicated. We by no means object to a repetition of really good figures, especially when they are taken from Monographs whose authors have made a special study of the organisms they represent, and are equal or perhaps superior to any that could be produced *de novo* from less satisfactory materials; but justice as well as courtesy to the originators of them, seems to us to demand that these be not deprived, even by implication, of their rightful title.

Review II.

1. *On the Organic Diseases and Functional Disorders of the Stomach.* By George Budd, M.D., F.R.S., Professor of Medicine in King's College, London, late Fellow of Caius College.—*London,* 1855. pp. 357.
2. *Digestion and its Derangements. The Principles of Rational Medicine applied to Disorders of the Alimentary Canal.* By Thomas K. Chambers, M.D., Fellow of the Royal College of Physicians, Physician to St. Mary's Hospital, and Lecturer on the Practice of Medicine at St. Mary's Medical School, London.—*London,* 1856. pp. 552.
3. *Stomach and Intestine.* By Dr. Brinton. In the 'Cyclopædia of Anatomy and Physiology,' parts 46 and 47.—*London,* 1855.

Every one who has paid any attention to the progress of medical science in this country, must be aware that Dr. Budd has for many years made the chylopoietic viscera a subject of special study. The present volume, 'On the Organic Diseases and Functional Disorders of the Stomach,' is almost entirely made up of lectures delivered either at the College of Physicians, or to the students of King's College, and published shortly after delivery in the 'Medical Gazette,' and the 'Medical Times,' with such additions and corrections as the author's subsequent experience has suggested. It is a work of a purely practical character.

Dr. T. K. Chambers has produced a book of a very different stamp. The first half of his volume is composed of a sketch of the microscopic anatomy and the physiology of the parts concerned in the process of digestion, concluding with a very interesting chapter on 'The Physiological Action of Substances submitted to Absorption in the Alimentary Canal,' from which we should have quoted freely, if we had not speedily traced the origin of its most important sections to articles originally published by the author in this Review, on 'The Use of Alcohol, Tea, Coffee, and other Accessory Foods.' He has freely availed himself of the scientific labours of Bidder and Schmidt (the eminent Dorpat physiologists), and of the investigations of their numerous pupils; and shows himself perfectly conversant with the works of Kölliker, Ecker, Bernard, Frerichs, Lehmann, &c.; he has thus succeeded in presenting to the English reader a correct history of the process of digestion, in so far as it has been elucidated by the most eminent physiologists and some of the most celebrated chemists of the day. The last half of the volume, 'On the Derangements of Digestion,' is composed of ten chapters, in which the following subjects find prominent places:—Changes in Parts common to the whole Alimentary Canal; Morbid Affections of the Mouth and Gullet affecting Digestion; Morbid Affections of the Stomach; Morbid States of the Small Intestines interfering with Digestion; Morbid States of the Pancreas; Morbid States of the Liver affecting Digestion; Morbid States of the Colon affecting Digestion; Flatulence; and Regimen.

In the following pages we shall follow the arrangement adopted by Dr. Budd, giving, as far as our limits will allow us, an analytical sketch

of his work; while, at the same time, we shall not unfrequently advert, by way of illustration, to the parallel investigations of Dr. Chambers.

After a brief notice of the special difficulties attending the study of disorders of the stomach, Dr. Budd enters somewhat fully into the consideration of the changes which that organ undergoes after death from the action of its own proper (gastric) juice. Our readers are doubtless aware that it was John Hunter who first announced that the stomach may be dissolved or digested after death by the secretion poured forth from its own walls. The first two occasions on which he observed this phenomenon were on men who were killed from fracture of the skull (one man dying outright, and the other in a few hours); and he afterwards met with it in a man who had been hanged. He found softening of the stomach of the same kind in some of the animals on which he was making experiments with regard to digestion, and he frequently observed a similar appearance in different varieties of fishes, especially such as were killed when the stomach was more or less filled. From these and similar cases, he concluded that digestion of the stomach is most common, and takes place to the greatest extent, in persons who die violent deaths; but (he observes) there are few dead bodies in which the stomach, at its great end, is not in some degree digested. The next important step was made by Spallanzani, who, in repeating and varying Hunter's experiments, discovered the important fact, that a certain amount of heat is requisite to develop the solvent action of the gastric juice. Lastly, the experiments of Wilson Philip, and Carswell tend to show that this change only takes place when death happens soon after a meal—that is to say, when the process of gastric digestion is actively going on. Hence the conditions on which this peculiar process depends are—1. That the stomach at the time of death should contain a certain quantity of acid gastric juice; and 2. That the body should be exposed for some hours after death to the temperature required for artificial digestion. The first of these conditions is generally fulfilled in healthy persons meeting with fatal accidents soon after a meal; and it has hitherto been commonly supposed that intense post-mortem digestion of the stomach only occurs under such circumstances. Dr. Budd, however, quotes a very interesting case, in which the great end of the stomach and the adjacent portion of the diaphragm were completely dissolved in the body of a gentleman who met with a violent death from fracture of the skull, at a period when the stomach was probably quite empty.* It is difficult in this case to understand how, without the ordinary stimulus of food, so much gastric juice should have been secreted as to have occasioned this destructive process of solution. Was it (he asks) determined by the shock of the accident, or by the subsequent irritation of the brain which the accident occasioned? Dr. Budd seems almost inclined to attach some weight to the view which Hunter early entertained but subsequently abandoned, that digestion of the stomach is especially apt to occur after death from fracture of the skull.

In certain diseases, gastric juice seems to be secreted when the stomach is empty, and consequently exists in the stomach, unmixed with food: and, moreover, there are certain catarrhal states of that organ in which lactic acid is freely generated from the saccharine principles of the food, and

* See pp. 16–17.

forms with the mucous membrane an efficient digesting mixture. In persons who die from (or with) these diseased conditions, digestion of the stomach may occur in as high a degree as in healthy persons killed by accident soon after a meal; and Dr. Budd even goes so far as to maintain that—"Not unfrequently the softening of the stomach may be predicted with tolerable certainty by a peculiar train of symptoms which result from the presence of free gastric juice or of a digesting acid in the otherwise empty stomach."

This occasionally happens in cases of simple ulcer of the stomach. It is often found, sometimes in a high degree, in persons, and especially in women, who die from phthisis: these patients generally having had, for some time before death, much disorder of the stomach, such as "pain and tenderness at the epigastrium, loss of appetite, thirst, frequent vomiting (the matters vomited being slightly acid), or frequent nausea." Dr. Budd suggests the probability that, in these cases, the flow of gastric juice in the empty stomach is excited by irritation of the lung. It frequently takes place in persons who die from inflammatory diseases of the brain—diseases which give rise to the same kind of secondary gastric disorder as tuberculous diseases of the lungs—as vomiting, nausea, pain in gastric region, &c. It is likewise of common occurrence in persons who die from typhoid fever (especially when there is much cerebral disturbance), and is occasionally met with in persons who die from cancer of the uterus, from peritonitis, or from other abdominal diseases leading to secondary functional disorder of the stomach; but it is in infants who die from the age of three months to two years, especially in cases of hydrocephalus and phthisis, and occasionally in deaths from exhaustion, consequent on the eruptive fevers or on improper diet after weaning, that softening of the stomach, in a high degree, occurs most frequently.

These facts have been observed by Louis, Cruveilhier, and others, but they supposed the softening to be the result of some disease which gave rise to no appreciable symptoms, and to occur during life. Dr. Budd, on the other hand, regards the softening as occurring in all cases after death, and holds that its degree depends, *cæteris paribus*, solely on the quantity of gastric juice in the stomach at the time of death; and the preceding remarks show that it is especially common in those diseases which have been long known to lead to secondary functional disorder of the stomach, accompanied either with an augmented secretion of gastric juice, or with secretion of gastric juice when that organ does not contain food, or with undue retention of the solvent fluid.

The next question discussed by our author is, How is this functional disorder brought about in these several diseases, and what is its real nature? We must content ourselves with stating his conclusions, that the secondary disorder of the stomach in these diseases is produced through the intervention of the nervous system; and that it affects the secreting apparatus, and not merely the muscular coat of the viscus in question. Nor can we follow him through his lucid description of the rarer forms of softening of the stomach by the gastric juice; we must restrict ourselves to his summing-up, which is as follows:

"The result, then, at which I arrive is, that the softening, with thinness, of the coats of the alimentary canal, described by Louis; the pasty or pulpy, and the

gelatiniform softening, of Cruveilhier; and the other varieties described by other authors, distinguished by the colour of the softened tissues, are essentially the same change; and that this change, whether it exist in the lower end of the œsophagus, or in the great end of the stomach, or in the fore part of the stomach only, or in any part of the small or the large intestine, is the result of *digestion* after death; like the softening of the great end of the stomach, remarked by Hunter, that occurs after sudden and violent death in the midst of health, and soon after a meal." (p. 46.)

After a few remarks on the importance of the subject, both in reference to medico-legal inquiries and to practice (because the study of it leads us to the knowledge of a peculiar and not uncommon form of indigestion, dependent mainly on the presence of free acid in the stomach and bowels), we proceed, in the third lecture, to Congestion of the Stomach.

The most simple kind of congestion is that which results from some mechanical impediment to the return of blood from the stomach to the heart. Dr. Yellowly's [Yelloly's?] well-known cases, showing the effect of death by strangulation in the induction of this kind of gastric congestion, are referred to, and one of them (that of Nicholson) quoted; and Dr. Budd proceeds to illustrate by a very forcible case, the close similarity in this respect between the effect of severe epileptic fits and the sudden strangulation inflicted by the hangman. Congestion of the stomach, in a less marked degree, always exists in persons with the hob-nail or gin-drinker's liver, in consequence of the impeded passage of the gastric venous blood through the portal vein. Dr. Budd states that he has met with several instances of convulsions in which death was very much hastened by hæmorrhage from this source, and in which the sound state of the stomach showed that the blood had exhaled from the unbroken surface of its mucous membrane. The same thing may happen in organic diseases of the heart, and in such diseases of the lung as interfere with the passage of the blood through the chest, the hæmorrhage being usually very slight, but occasionally profuse and violent, as in two cases of rheumatic pericarditis described by our author.

He then shows, from a review of analogous cases, that "it is in accordance with a general law, that when the stomach is kept in a state of passive congestion from a bar to the current of blood through the liver or chest, the secretion of the gastric juice is diminished, the stomach can digest less food, and requires longer intervals of rest." If, under these circumstances, indigestible food, or an excess of ordinary food, be taken, some of it remains undigested in the stomach, and irritates, and may even inflame, the mucous membrane. Hence in the treatment of the diseases which occasion this congestion of the stomach, and consequent feebleness of digestion, we should prescribe a sparing and easily-digestible diet, and, when the system can bear it, total abstinence from fermented drinks; if, however, alcohol seems necessary, it should be given sufficiently diluted; and (adds Dr. Budd) when mercury and diuretics are deemed expedient for the object of relieving the embarrassed circulation, their action on the stomach should be carefully watched.

We have hitherto spoken only of the simplest kind of congestion, namely, the vascular turgescence arising from a mechanical impediment to the return of the blood from the stomach to the heart. It may, however, result from very different conditions. To say nothing at present of

the congestion that may arise from inflammation of the stomach, or from the growth of a cancer, or from other structural changes, a not unfrequent origin of this affection must be sought in an unnatural condition of the tissues which the blood nourishes, and likewise in an unnatural condition of the blood itself. Under this last head, Dr. Budd places the congestion of the stomach, and the consequent hæmorrhage which sometimes occurs in women from a stoppage of their monthly courses, and which are not unfrequently observed in malignant cholera and in yellow fever, and now and then, though very rarely, in the course of typhoid fever. Lastly, amongst the causes of congestion of the stomach, and consequent hæmorrhage, we must notice arrest of the biliary secretion, and enlarged spleen; in the latter case we find that the hæmorrhage is preceded or attended by ascites, by enlargement of the superficial veins of the belly, and by other symptoms, which show that the passage of the blood through the liver is greatly impeded. Whether, however, the seat of obstruction lie in the liver or in the spleen itself, is not yet fully determined.

The next two lectures are devoted to inflammation of the stomach. We shall briefly notice the most important forms of this affection, beginning with the simpler and proceeding to the more severe varieties. (1) One of the simplest and most frequent forms of inflammation of the stomach, is that which is brought on by excess in eating or drinking, and especially by the ingestion of substances hard of digestion and of alcoholic drinks. Dr. Budd quotes freely from Dr. Beaumont's experiments on Alexis St. Martin, which throw much light on this form of inflammation:

"Many instances," says our author, "are on record, in which a person previously in good health has died very speedily and unexpectedly after a gluttonous meal of some indigestible substance, and apparently in consequence of the pain which the over-distension and irritation of the stomach occasioned. In such instances, the immediate cause of death is faintness, or stoppage of the heart's action, under the influence of the pain; and I believe that in most of them the heart was previously unsound, or the power to bear up under pain or other distressing sensations had been weakened by intemperate habits." (p. 92.)

(2) A higher degree of inflammation, brought on in the same way—namely, by direct irritation of the stomach, is seen in persons who have swallowed hard and insoluble substances, or irritant or corrosive poisons. Under this head we find a notice of the case recorded by Dr. Marcet, of a sailor who, in imitation of a juggler, swallowed fourteen clasp knives, and lived between three and four years afterwards, although at first, and at intervals subsequently, he suffered very severely from pain in the stomach, vomiting, &c.; and of the still more remarkable case recorded by Dr. Spry, of the man who swallowed molten lead during the fire which consumed the Eddystone Lighthouse in 1755. In attempting to throw water upon the fire above him, and in looking up to watch the result, a quantity of molten lead fell upon his head and face, severely burning those parts, as well as his neck and shoulders. From that moment he had a violent internal sensation, and imagined that a quantity of the lead had passed down his throat into his body. He and his companions were rescued and taken to Plymouth, where he was attended by Dr. Spry, who, however, would not believe his statement regarding the lead he had swal-

lowed. He constantly took his medicine, and swallowed many things, both liquid and solid, till the tenth or eleventh day, after which he suddenly grew worse; and on the twelfth day, being seized with cold sweats and spasms, he soon afterwards expired. On examining the body, Dr. Spry "found the diaphragmatic upper mouth of the stomach greatly inflamed and ulcerated, and the tunica in the lower part of the stomach burnt; and from the great cavity of it took out a great piece of lead, weighing 7 oz. 5 drs. 8 grs." He transmitted an account of the case to the Royal Society, "but that learned body, thinking the circumstance very unlikely and extraordinary, and doubting the truth of it, the reading of the paper was deferred until a further elucidation was received." Dr. Spry, finding his word thus doubted, took the wisest course that was open to him, and performed a series of equivalent experiments on dogs and fowls by pouring molten lead down their throats. These experiments clearly showed that there was no impossibility in the case he had recorded being perfectly true; and on forwarding them to the Royal Society, his original paper was read, and afterwards published in the 'Transactions.' Similar experiments were made a few years ago in France, by Bretonneau, who injected boiling water into the stomachs of dogs. Four of these dogs were killed on the third day, and one on the seventh day, after the operation; the stomachs were found to be more or less in a state of gangrene, but the animals were cheerful and played together after the first two days.

Similar to these are the numerous cases recorded in our medical literature, in which strong mineral acids have been taken, and in which the patients have often survived for a considerable period.

3. Another condition giving rise to inflammation of the stomach, is long-continued abstinence. Although this fact has been indistinctly perceived since the time of Hunter, we are indebted to MM. Andral and Gavarret for distinctly establishing it in a series of observations which they instituted, some years ago, on the influence of various conditions on the blood. In one of their experiments (quoted by Dr. Budd), three dogs were employed: the first was entirely deprived of food and drink, and lived twenty-one days; the second wa deprived of food, but allowed to drink water, and lived twenty-five days; and the third was allowed a small quantity of soup every morning, and lived thirty-three days. The third dog was the only one in which the stomach was not ulcerated; and the abnormal redness of the mucous membrane of its stomach was less general and less vivid than in the others. Its case seems to show that even a small quantity of food, if it contain all the necessary elements of nutrition, may hinder those destructive changes which are caused by total abstinence. Although "evident signs of inflammation" were found by Andral in the stomachs of the animals on which he experimented, it does not appear, from the cases quoted by Dr. Alfred Taylor, in his 'Manual of Medical Jurisprudence,' that inflammatory redness of the stomach is by any means invariably present in deaths from starvation in the human subject.

Somewhat similar are the effects produced by a long persistence in food which, whatever be its quantity, is not sufficiently varied for healthy nutrition. This is well shown by the experiments of Magendie, and of

the committee of the French Institute, which showed that dogs kept exclusively on water, with the addition of oil, sugar, fat, or even of albûmen, fibrin, or gelatine, soon die of starvation, just as when kept on water alone; and is painfully confirmed by some of the earlier Reports of the Inspectors of Prisons, especially in reference to a purely bread-and-water diet. Dr. Budd is inclined to think that a rigid diet, too long persisted in, in the early stage of continued fever, has often been productive of serious gastric disorder.

4. Lastly, inflammation of the stomach may be excited by the presence of some noxious matter in the blood,—as, for instance, arsenic or other irritant substances introduced from without, or morbid matters that are, under special circumstances, generated in the body. In the latter category we may place the poisonous matters which seem to be generated in the system in yellow fever and in cholera, and very probably in certain gouty states of the system.

Dr. Budd's remarks on the treatment of inflammation of the mucous membrane of the stomach are sound and judicious, but present no novelty. The fundamental point is to give the stomach sufficient intervals of rest, and to avoid irritating it by physic or food. In slight cases, all that is usually necessary is restriction for a few days to a sparing diet of farinaceous substances and milk, and the free use of cooling drinks; in more severe cases we must apply leeches to the epigastrium, and allow the patient to sip iced water, or to suck small pieces of ice, swallowing the water as the ice dissolves.

Towards the conclusion of this chapter we find a notice of that comparatively rare form of inflammation of the stomach in which coagulable lymph is effused into the sub-mucous cellular coat, and, hardening and contracting, forms a dense gristly tissue, binding the mucous membrane to the coats beneath. Lymph effused in this way often forms a gristly hard ring around the pyloric extremity, which acts as a permanent stricture. This form of disease seldom occurs till about the age of forty, and is almost invariably the result of spirit drinking. The symptoms of obstruction thus produced are the same as those occurring in cancer of the pylorus; it may, however, often be distinguished from the latter disease, (1) by its slower progress; (2) by the absence of hæmorrhage, which frequently occurs in cancer; (3) by the absence of any palpable tumour, which can often be detected in the latter disease; and (4) by the fact that it almost always occurs in spirit drinkers.

The treatment consists in the application of leeches and blisters, and the prescription of a rigid diet, as long as the inflammatory process is going on; subsequently the treatment can be only palliative; a most important point is, make the patient altogether give up the use of spirits.

The two next chapters treat of Ulceration of the Stomach—an affection which has received the names of *simple*, *chronic*, and *perforating* ulcer.

The stomach in most of these cases appears healthy, except for the existence of a single deep ulcer on its inner surface; this ulcer is seldom larger than a shilling, is generally circular or oval, and its edges are as sharply cut as if a portion of the mucous membrane had been punched out. Sometimes only the mucous membrane is destroyed; in other cases

the ulcerative process gradually eats through the other coats in succession, so as finally to give rise to perforation, in which case the contents of the stomach escape into the sac of the peritonæum; in these cases the external is much smaller than the internal aperture. The ulcer is generally situated along or near the lesser curvature of the stomach; usually nearer the pyloric than the cardiac orifice; and much more frequently on the posterior wall of the stomach than on the anterior. It is very seldom that more than one ulcer exists: of 79 cases noticed by Rokitansky, the ulcer was solitary in 62; of the remaining 17 cases there were 12 in which two ulcers existed, 4 in which there were three ulcers, and 1 in which there were five.

These ulcers are sometimes met with cicatrized; and sometimes, if the ulcer has been a large one, the process of healing, by the contraction that attends it, permanently alters the form of the stomach. Dr. Budd refers to two preparations in the King's College Museum, "in each of which the stomach is divided into two pouches, as if by a string passed transversely round it, looping up the greater curvature towards the lesser."

Our knowledge regarding the circumstances and special causes giving rise to ulceration of the stomach, is very defective; and none of the authors whose works we are now reviewing have added much to our information on these points.* That this simple or chronic ulcer of the stomach is not a rare affection, is shown by the data given by Jaksch, Dittrich, Willigk, and Dahlerup, in Germany; and by T. K. Chambers, Gairdner, Habershon, and Handfield Jones, in our own country, and which are quoted by Dr. Brinton, in page 160 of the seventeenth volume. It is more common in women than in men.† It hardly ever occurs before the age of puberty. (Dr. Budd once met with it in a girl aged fourteen and a half.) It is most common in the earlier portion of middle life; but has been met with up to the seventieth year. It seems to be relatively more frequent amongst the poor than amongst the rich, although it is confined to no class of society; and it is especially often found in maid-servants, between the ages of eighteen and twenty-five. Dr. Budd may possibly be correct in stating that "the ulcer has not been found in conjunction with, or in sequel to, any other disease, with such frequency as to lead us to conclude that it has any intimate connexion with it;" but he might with propriety have noticed the special frequency with which it occurs in pulmonary tuberculosis, and have alluded to the remark made by Rokitansky, that several of the patients whose bodies he examined, traced the origin of their gastric disease to intermittent fever. Moreover, Jaksch believes that childbed predisposes towards it.‡ Dr. Chambers, whose 'Decennium Pathologicum' testifies how fully he has availed himself of the information yielded by the St. George's Case Books, agrees, however, with Dr. Budd, in considering ulceration of the stomach as always an independent disorder, and not a symptom or a consequence of other affections. In short, it would appear that we have as yet no clue to the real cause of the disease, although the facts, that it is commoner amongst the poor than amongst the rich, and

* When these pages were written, Dr. Brinton's Essay On Ulcer of the Stomach, which appeared in the January number of this Review, was not published. See especially pp. 160–62.
† See Dr. Brinton's Essay, p. 161.
‡ In relation to this point we may refer to Dr. Brinton's Essay, p. 180.

that it is more frequent amongst young unmarried maid-servants than in other classes, would favour the inference that a state of anæmia predisposes to it. Why it hardly ever occurs before the age of puberty—why the ulcer is almost always single—and why it is always in the pyloric division of the stomach, and most commonly in or near the lesser curvature,—are questions which at present we have no means of answering.

Ulcers in the stomach are very difficult to heal, and in this respect they contrast strongly with corresponding lesions in the small and large intestine, which especially occur in fever and dysentery. The reasons for this difficulty of healing are—(1) the great change of volume to which the stomach is liable two or three times a-day, according as it is full or empty; (2) the constant churning motion which takes place in the stomach during digestion; (3) the mechanical and other irritation of the sore caused by the various substances taken as food and drink; and probably (4) the irritating action of the gastric juice, which, although it exerts no injurious effect on healthy mucous membrane, may dissolve and remove the plastic lymph which is effused at the bottom of the ulcer for the purpose of repairing the lost substance. It is thus (says Dr. Budd) "that a small ulcer, which causes no constitutional disturbance, which may not even much impair the nutrition of the body, and which, if situated in a lower portion of the same canal, might soon heal, becomes so serious a disease;—how it leads so continually to long-continued suffering and death."

Before turning to the symptoms of ulceration of the stomach, we may notice the natural end of this disease, if it runs its full course. There are three distinct ways in which it may prove fatal. (1) By perforation. "In the decennial period at St. George's Hospital, ending Dec. 31st, 1850, there died 24 cases of malignant disease of the stomach, and in these perforation was found thrice; there were 19 cases of simple ulcer. and perforation occurred in 9 of these. The difference is sufficiently marked to make the smallness of the numbers of no importance."* (2) By hæmorrhage, in consequence of the ulcer eating into one of the arteries of the sub-mucous cellular tissue. And (3) in rare cases, by mere exhaustion.

Sometimes ulceration may exist without any marked local symptoms. In 3 of 15 cases referred to by Dr. Chambers, in which ulceration of the stomach was present, but where death occurred from other causes, there were no local symptoms referable to that organ; and in the others, none were prominent enough to attract attention, excepting that "one patient had a pain in the left hypochondrium (which, by the way, was *not* the situation of the ulcer), and a fanciful appetite, but no vomiting, pyrosis, or any other mark of aggravated dyspepsia."†

Several explanations may be given of the absence of local phenomena. Dr. Chambers probably suggests the correct one—namely, that the want of common sensibility in the stomach makes it irresponsive to our modes of examination; and this view is confirmed by the fact, that when pain does exists, it seldom points out the exact locality of the injury.

In the great majority of cases, especially when the disease has lasted some months, the symptoms are generally well marked and significant

* Chambers, Digestion, &c., p. 402. † Ibid., p. 404.

enough. We will notice them in the order in which they are discussed by Dr. Budd. "The most constant symptom is pain in the stomach, which is generally referred to a small spot, and is more severe after meals, when the stomach is distended, and when its vermicular movements are going on." We regret that our author says nothing regarding the interval of time that generally elapses between the ingestion of the meal and the commencement of the pain, as definite information on this point would be valuable. According to Dr. Chambers, it generally begins within a quarter of an hour, and sometimes even attains its maximum within five minutes after the termination of the meal. The pain is most marked when the food or drink is of a higher temperature than the body; it is likewise much increased by moving about after meals. Dr. Chambers attaches great weight to the fact that the pain of an ulcer in the stomach is always increased by pressure, especially the local pressure of one or two fingers. The pain, moreover, in this disease is variable, sometimes being very severe for days or weeks, and then suddenly ceasing; while in malignant tumours, the pain, though not always severe, is almost always constant after it has once begun. There is also occasional eructation of a sour fluid, and sometimes vomiting. Dr. Chambers (in a memoir published in the 'London Journal of Medicine' for 1852) gives two cases in which the vomiting was so severe as to cause death; and he points out the importance of watching it closely wherever it occurs; for when ulceration is the cause of it, streaks of blood will seldom fail to be found some time or other, and they are "of the highest value in a diagnostic point of view, because they are extremely rare in other diseases which may be mistaken for simple ulcer." Often, however, the hæmorrhage is more abundant, as we have already mentioned, and in these cases it is not unfrequently preceded for a day or two by an increase of pain. Two or three pints of black clotted blood are sometimes vomited at once, while an additional quantity is carried off by the bowels, but the hæmorrhage seldom lasts more than a day or two. When it has once occurred, it is very apt to occur again, generally after the lapse of some months, but sometimes not for two or three years. Dr. Budd lays great stress on the fact, that "in persons under thirty, the only organic disease of the stomach that gives rise to profuse hæmorrhage, with very few exceptions, is ulcer." In persons above that age, vomiting of blood, preceded by disordered and painful digestion, may likewise arise from cancer; but Dr. Budd believes that the source of the hæmorrhage may be determined from the following considerations:

"Cancer of the stomach in most cases originates at the pyloric or cardiac orifice, and in some degree narrows or obstructs it. It also gives rise to a tumour, which at the end of some months is generally palpable enough · and it *always* interferes greatly with nutrition, causing progressive, and after a time extreme, wasting; while simple ulcer seldom produces any of these effects."*

Moreover,

"A simple ulcer may continue almost stationary—at any rate with little change in the symptoms—for twenty years. Cancerous disease, on the contrary, constantly and steadily progresses; the symptoms become week after week more marked; and although life may be protracted, especially in colloid cancer, for four or five years, the patient generally dies, much emaciated, within twelve months."†

* Budd, op. cit , p. 156. † Ibid.

The treatment of ulcer of the stomach is more dietetic than medicinal. We alluded in p. 36 to the circumstances which impede the healing of the ulcer: it is by lessening as much as possible their unfavourable influences that recovery is best promoted. The patient should eat little at a time, and the food should be of the least irritating kind. According to Dr. Budd, milk, and compounds of milk with farinaceous substances of the most nutritious kinds (such as bread, macaroni, semolina, biscuit-powder, Indian meal, and oatmeal, in preference to arrow-root and other substances consisting mainly of starch), constitute the most appropriate diet in these cases; and if the milk be good, and the other substances duly varied, a person may be kept on such food, in conjunction with tea and sugar, for a long time, without any impairment of strength. Dr. Chambers especially recommends iced milk, with the addition of from one-quarter to one-third of lime-water; if two or three tablespoonfuls of this mixture be taken at short intervals, regular meals are rendered unnecessary, for as much as a couple of quarts may easily be digested in the day. Moreover, he is strongly opposed to Dr. Budd, on the propriety of allowing sugar. "Those who have any ulcers in the digestive mucous membrane, should as cautiously avoid it, as those with tender teeth."* As the condition of the stomach improves, calves'-foot jelly, beef-tea (preferable, we think, if cold), or the yolk of a soft-boiled egg, may be tried; and if these can be borne without exciting pain or vomiting, the diet may be gradually enlarged.

Next in importance to a proper dieting, Dr. Chambers ranks the application of leeches (three at a time, about twice a-week) to the region of the stomach; they do not (he observes) weaken the patient, for weight will almost always be found to be gained during their use, and they are a most powerful means of preventing local congestion, of getting rid of the effete venous, and inducing a passage of fresh arterial, blood through the capillaries of the neighbourhood. It is singular that Dr. Budd alludes neither to local bleeding nor to blistering; the latter, applied to the spine, often relieves the dorsal pain which not unfrequently accompanies this disease.

When the irritation of the ulcer causes an effusion of acid gastric juice into the empty stomach, the trisnitrate of bismuth is of service. Dr. Budd recommends from five to ten grains, suspended in water by means of compound tragacanth powder and syrup, two or three times a-day, a quarter of an hour before meals, and a dose of magnesia at night; while Dr. Chambers holds that "it must be administered in large doses of from fifteen to twenty grains, or no advantage follows, and may freely be increased to two scruples or a drachm, if necessary."

Dr. Budd allows the patient to swallow small lumps of ice when the stomach is very irritable; and to take opium (which is best given in its crude form, and in pills) when the pain at the stomach is very harassing, and the nights are restless. He makes no allusion to astringents (except in the treatment of hæmorrhage), a class of remedies which Dr. Chambers declares "are often of signal benefit." Amongst these he assigns the first place to the newly-discovered salts of metals, whose constitution and form are the same as alum, especially to "iron alum."

* Chambers, op. cit., p. 411.

"Three or four grains of this salt, taken thrice a-day, give an immediate relief to the pain; and this medicine has also the advantage of adding to the blood a metallic constituent, usually much required in these cases, and improving the anæmic condition. As a change, gallic acid, nitric acid, and bitter barks, may be employed; and in these cases, as in external ulcers, I have found great benefit from variety. The original medicine is often much more efficacious after a few days' change to another. Nitrate of silver, given as usual, in pills, by itself, has disappointed me in the treatment of suspected ulcer, and indeed in all gastric complaints. . . . . The blunting of the sensibility of the sore surface by medicine enables us sooner to return to hitherto unborne articles of food, than had drugs not been used, and thus much to shorten the convalescence."*

Dr. Budd observes, that "when the bowels are much confined, an aloetic or a compound colocynth pill should be given: these medicines irritate the stomach less than castor oil, rhubarb, or the saline purgatives." We cannot altogether concur with him on this point. We have more than once seen great aggravation of the symptoms caused by colocynth pills, in cases which could bear castor oil, associated with a few drops of laudanum, with little gastric disturbance; indeed, we regard castor oil and enemata as affording by far the best means of counteracting the constipation that so often occurs in this disorder.

We shall conclude our notice of the therapeutics of ulcer of the stomach by quoting Dr. Budd's directions for treating the hæmatemesis which so frequently occurs:

"The means most likely to restrain the hæmorrhage are, ice swallowed in small quantity or applied to the epigastrium, rest in the horizontal posture, *prolonged fasting*, and medicines which have an astringent or styptic action—such as oil of turpentine, acetate of lead in conjunction with opium, alum, and tannin."†

Of these, oil of turpentine is perhaps the most trustworthy; it is best given in *cold* water, in doses varying from ten to twenty minims, repeated more or less frequently, according to the urgency of the symptoms.

Want of space compels us to omit any notice of Dr. Budd's observations on Perforating Ulcer of the Duodenum, and on Minute Superficial Ulcers of the Stomach. We are thus brought to the eighth lecture, on Cancer of the Stomach; and we quote, with some abbreviations, his observations on the diagnosis of this disease.

"The existence of cancer of the stomach is the more difficult to ascertain, from our not knowing its causes or any circumstances in which it is especially apt to occur. It does not, indeed, often occur before the age of thirty-five;‡ but in persons beyond this age it is met with, and with no observed differences as to frequency, in all conditions of society. . . . . In persons beyond the age of thirty-five, there are no circumstances that give unusual significance to symptoms; and until the disease has lasted some time, the symptoms have seldom any characters that are peculiar or especially significant. Pain or uneasiness, referred to the stomach, and increased by food, sour eructations, occasional vomiting, and lowness of spirits—which are often the only symptoms noticed for some weeks, or even months—may all arise from simple ulcer of the stomach, and from many other conditions.

* Op. cit., pp. 412–13. † Budd, op. cit., p. 142.

‡ Andral relates a case in which a woman died of this disease at the age of twenty-two, and in whom it seemed to have begun before the age of twenty; and Dr. Budd has met with an instance of its occurrence in a woman at the age of twenty-six; but such cases are extremely rare.

"After some time, the disease is easier to detect. The circumstance, that the symptoms have continued, or rather that they have got gradually worse, in spite of a restricted diet, and of other means which usually relieve such symptoms when they are the effect of superficial inflammation of the mucous membrane, or of mere functional disorder, leads to the inference that they result from organic disease; while the progressive loss of flesh, and the faded look, and often a morbid moroseness or despondency, must excite a strong suspicion that this disease is cancer. Such a suspicion will be confirmed if much mucus, especially mucus mixed with brown or black flakes, should be thrown up from the stomach. When the mucous membrane is invaded by cancer in considerable extent, this frequently happens; but in simple ulcer of the stomach, the disease most likely at this time to be mistaken for cancer, it happens but rarely. . . . . .

"After the disease has existed for some months, and the patient is much wasted, a tumour may, in most instances, be felt in the region of the stomach. When such is the case, and when the discovery of the tumour has been preceded by the symptoms I have mentioned, and the patient is of an age when cancer of the stomach is common, little doubt can remain that the disease is cancer. Vomiting of a large quantity of matter of a soot-black or dark-brown colour, like coffee-grounds, would of course render this inference still more sure.

"In a person who has led a temperate life, the same inference may be drawn when, without any palpable tumour, the symptoms show clearly that the pylorus is much obstructed. . . . . . In distinguishing cancer of the stomach, it is also very important to consider the time the disease has already lasted, and the actual condition of the patient with reference to it. In cancer the disease makes continual progress, the patient grows gradually thinner, and, in three cases out of four, dies of exhaustion within twelve months; in a very large proportion of cases, within two years. . . . . If the disease, therefore, has existed for several years, or even for many months, without much loss of flesh, the chances are greatly against its being cancer."*

Dr. Budd's observations on the possibility of diagnosing the different kinds of cancer are sound and judicious. We are assisted in our diagnosis by the following considerations:

1. Medullary cancer grows much more rapidly, and becomes sooner and more widely disseminated than scirrhous and colloid cancer.

2. Scirrhous and medullary cancer of the stomach usually (if they extend) affect the liver; while colloid cancer more commonly leads to secondary cancerous tumours in the mesentery.

3. Medullary cancer and scirrhus cause much more pain and constitutional disturbance than colloid cancer.

Both Dr. Budd and Dr. Chambers lay down very good rules regarding the diet in these cases, but as they contain nothing particularly novel, we proceed to extract a few hints from Dr. Budd, regarding the treatment of special symptoms.

When there is an excessive secretion of mucus or gastric juice, bismuth may be administered before meals with advantage. An excess of acid in the stomach (which, by the way, often occasions a great repugnance to food) may be neutralized by lime water or magnesia. Associated with the development of the acid, there is usually an evolution of carbonic acid, resulting from a process of gastric fermentation, which may be checked to a certain extent "by brandy, and by dill-water and other aromatics, combined with bismuth, or with alkalies, when these are necessary." When there are fetid eructations, containing sulphuretted hydro-

* Op. cit., pp. 176–8.

gen, creosote pills (containing from a quarter to half a minim) may be given with each meal; or a few grains of bisulphite of soda; or some finely-powdered wood-charcoal. Finally, "to alleviate pain, and to allay general nervous irritability, the medicines most in repute, and probably the best, are conium and belladonna, which have the advantage of not confining the bowels and checking the secretions, as opium does."

The lecture which we have just noticed forms the conclusion of what may be regarded as the first half of Dr. Budd's volume—that, namely, which treats of the *organic diseases* of the stomach; the greater part of the remaining lectures being devoted to the *functional disorders* of that organ.

It has been known from time immemorial, that sympathetic disorders of the stomach may arise from irritation elsewhere.

"Functional disorder of the stomach," says Dr. Budd, "may result not from organic disease of the stomach merely, but from organic disease of other organs; and that, not by the constitutional disturbance which this disease sets up, or by any change it may cause in the state of the blood, but by an influence transmitted through the nerves. . . . . Irritation of the lung, or of the brain, or of the liver, or of the uterus, from certain kinds of organic disease, frequently leads, as is well known, to *sympathetic* vomiting—that is, to vomiting caused by nervous influence reflected from the seat of disease upon the muscles which perform this act. The matter vomited in such cases is frequently acid, even when digestion is not going on . . . . whence we may safely infer, that the reflex nervous influence excites, not merely the act of vomiting, but also, in many cases, a secretion of gastric acid." (pp. 185-6.)

This sympathetic gastric disease is very commonly seen in phthisis. In this disease, vomiting generally occurs sooner or later, and the functions of the stomach are otherwise disordered. Disorder of the stomach of a similar nature sometimes originates in the liver, especially from the irritation caused by the passage of gall-stones, or by hepatic abscess. It may likewise result from disease of the brain, especially from inflammation of that organ or of its membranes. Andral, who has investigated this subject with much care, and whose results are quoted by Dr. Budd, observes, that "vomiting, or at least nausea, very frequently attends acute inflammation of the membranes of the brain. These symptoms show themselves almost exclusively in an early stage of the disease, and they often mark its onset." Gastric disorder of this kind results very frequently from irritation or organic disease of the uterus. It has been observed, when in tying a polypus, a portion of the uterus has been included in the ligature; it occasionally occurs in women afflicted with cancer of the uterus —an effect, doubtless, of reflex nervous influence; and it is sometimes observed when miscarriage is about to take place; and in conjunction with chronic ulcer of the neck of the womb. In children, the irritation of teething is a frequent source of this variety of gastric disorder.

"The most effectual remedies for the disorder are,—

"1. Sedatives, and other means which lessen the irritation from which the gastric disorder springs.

"2. Alkalies and astringents.

"Alkalies sometimes give immediate relief, by neutralising the acid which the stomach contains: astringents alleviate the disorder rather more slowly, but for a longer time, by restraining the undue and untimely secretion.

"The insoluble antacids—magnesia and chalk—are well suited to this disorder. They serve to neutralize any excess of acid that may be in the stomach; and given under these circumstances, have an astringent action on the surface besides.

"Bismuth has a remarkable effect in restraining undue secretion in the mucous membrane, and may often be given with advantage, either alone or in conjunction with magnesia or chalk.

"When the disorder continues, the diet should consist chiefly of milk and farinaceous food, and little should be eaten at a time. Alcoholic drinks and all stimulating articles of food seldom fail to aggravate the disorder, and should be strictly forbidden.

"If symptoms indicating an inflammatory state of the stomach should occur—namely, a sense of heat in the stomach, and pain excited by food, tenderness at the epigastrium, and a white coat on the tongue, a blister or mustard-poultice may be applied to the epigastrium; and the stomach may be cooled, and be rendered less irritable, by swallowing occasionally, and especially after meals, a small lump of ice.

"If constipation exist, it may be remedied by medicines, such as pills of colocynth or aloes, which do not much offend or oppress the stomach.

"In some cases of this sympathetic disorder all these means are unavailing: nothing will stop the vomiting while the original irritation exists."*

Dr. Budd's tenth lecture is devoted to a subject which is also ably discussed in Dr. Chambers's volume—namely, Deficient Secretion of Gastric Juice. A too scanty secretion of the gastric juice may arise from various causes. According to Dr. Chambers, "There is no defect, moral or physical, so frequently handed down from parent to offspring, as the inability to form a sufficiency of this secretion." Next to hereditary predisposition, we must place overwork of the mind, prolonged anxiety, ambition, &c.; and finally, gluttony, drunkenness, and, in a lesser degree, indolent and sedentary habits, and the consumption of more food (without positive gluttony) than the system, with so little tear and wear, requires. From any of these, and probably from various other causes (as, for instance, glandular degeneration, a subject which has recently been most ably investigated by Dr. Handfield Jones), it may happen that the gastric juice is not secreted in sufficient quantity for the purpose of digestion, and that consequently indigestion will ensue.

The signs of a deficiency of the gastric juice are (according to Dr Chambers):

1. An arrest of the food in the stomach.
2. Distress after eating albuminoid substances.
3. Decay of albuminoid substances in the alimentary canal, and consequent fetid gases arising from that decay.
4. The appearance of unaltered muscular fibre in the stools.

The food in these cases remains undigested, or only partially digested, in the stomach, for a much longer time than the normal period—sometimes for twelve or even twenty-four hours; and during this slow process there comes on a sense of weight or uneasiness at the pit of the stomach, which gradually disappears as the food becomes dissolved and escapes from the stomach.

"If (says Dr. Budd) portions of food remain undigested many hours, they irritate the lining membrane of the stomach, and cause headache, a slightly furred tongue, and feelings of general disorder.

* Budd, op. cit., pp. 205–6.

"Not unfrequently this irritation of the stomach checks secretion in the liver, so as to render the complexion somewhat sallow; and in nervous persons who dine late it greatly disturbs the sleep.

"When, from permanent weakness of the stomach, the digestion is habitually feeble, the body after a time is imperfectly nourished—the blood is poor in globules, the circulation is feeble, the extremities are apt to be cold, the spirits are depressed, and the various *powers* of the body decline." (p. 212.)

The remedies for habitual slow digestion, consist fully as much in the proper regulation of the diet and general habits, as in purely medicinal agents.

In slight cases a little comparative starvation will do no harm, but when the affection is chronic, the patient may not be in a state to bear starving; and in fact the disease might be aggravated by thus lowering the system. Under these circumstances, Dr. Chambers (p. 373) lays down the following rules:—1. To let the albuminoid food be as liquid as possible. 2. To let the quantity requisite for the day's consumption be taken at frequent short intervals; and (if it is likely to turn sour) 3. To guard it with alkalies. It is unnecessary to observe that Dr. Chambers's third rule is diametrically opposed to the ordinary mode of proceeding.

"I need hardly say (observes Dr. Budd), that when digestion is slow and feeble, care should be taken not to give, at the time of meals, or while digestion is going on, alkalies, the alkaline salts, or other medicines that suspend or enfeeble the action of the gastric juice." (p. 214.)

By following Dr. Chambers's third rule, we may get the food to pass unaltered into the intestines, and by trusting to their digestion, spare the stomach without starving the patient. We have great pleasure in directing the attention of our readers to these rules, not only because we believe them to be of sound practical value, but because they afford an excellent illustration of the direct service rendered by physiology to the treatment of disease. If it had not been for the physiological experiments of Bidder and Schmidt, and of their pupil Zander, we should never have known that the intestinal juice possesses the remarkable and unique property of dissolving and rendering fit for absorption, not only flesh and the other albuminoid bodies, but also starch —in short, that it unites in itself the powers of the acid gastric juice and the alkaline pancreatic fluid.* The following recommendations are also based on the physiological observations of Bidder and Schmidt:

"The gastric juice may in part be replaced by water drank; and hence we often find a draught of this liquid, an hour or two after meals, will remove the discomfort arising from difficult solution of meat meals. It is not merely the dilution of pungent irritating matters which is effected by these means, but the solution, and consequent absorption, of the delayed nutrimentary mass.

"The comfort arising from drinking a short time after meals, has originated the custom of tea and coffee soon after the evening dinner. The liquids taken are, however, not nearly so well suited to the purpose as pure cold water is, and many sufferers from sluggishly-secreted gastric juice will find from this latter beverage a relief which tea or coffee can never give.

"The water may be made pleasanter by being iced, without any consequent injury to digestion, for we know, from Drs. Bidder and Schmidt's experiments,

* For further information on the subject of intestinal digestion, we may refer to Lehmann's Physiological Chemistry (published by the Cavendish Society), vol. iii. pp. 314–17; and to Dr. Chambers's volume, pp. 138–43.

that the freezing temperature does not arrest the functions of the gastric juice, and probably, too, the cold gives a tone to the stomach which its torpid congested condition requires.

"Another addition to the water, not unpalatable, is a few teaspoonfuls of a solution of the superphosphate of lime, which is at the same time a condiment or fillip to digestion, and a complementary food.

"A lemon-water ice is another agreeable substitute for tea."*

The solution of the superphosphate of lime may be prepared, as Dr. Chambers informs us in a note, "by adding to the pharmacopœial solution of chloride of calcium, the rhombic phosphate of soda, and then redissolving the precipitate formed, by the addition of phosphoric acid. The chloride of sodium in the solution only makes it the more sauce-like."

With regard to medicines, Dr. Budd especially mentions ipecacuanha, rhubarb, and cayenne pepper, as substances which, by stimulating the lining membrane of the stomach, cause an increased flow of gastric juice.

"From half a grain to two grains of ipecacuanha and three or four grains of rhubarb, or a grain of capsicum with three or four grains of rhubarb, in a pill, may be taken before dinner, or before breakfast and before dinner. . . . . As a remedy for frequently-recurring slowness of digestion, ipecacuanha seems to me to be more effectual than any of the other stimulants. . . . . Where digestion is habitually slow and feeble, much benefit of more lasting kind may frequently be derived from the muriatic or the nitro-muriatic acid, taken for some weeks together, half an hour or three parts of an hour before the principal meals." (pp. 213-14.)

Dr. Budd concludes this lecture with a notice of some of the varieties of gastric fermentation. We extract his observations on one of the most important of this class of affections.

"Severe attacks of vomiting and purging, commonly designated English cholera, seem often the result of fermentation or putrefaction of food in the stomach, by which some highly-irritating matter is formed. If I may judge from my own experience, such attacks generally come on in the evening, soon after dinner, or at night, soon after supper, and are much more frequently consequent on a meal, of meat or cheese than on the eating of fruits, to which, perhaps from the greater frequency of the disorder in autumn, they are generally ascribed. I have found no remedy so effectual in checking the disorder as pills composed of creosote and opium." (pp. 221-22.)

The eleventh lecture is devoted to the consideration of that form of fermentation in which *sarcinæ* are developed in the stomach. The remedy on which he places the most reliance—and we can personally support his judgment on this point—is the bisulphite of soda, of which a dose varying from fifteen grains to a drachm may be given, dissolved in water, two or three times a-day. In one of his cases the disorder was much mitigated by large doses of common salt.

In his twelfth lecture, Dr. Budd treats of Indigestion arising from Defective Action of one of the Excreting Organs, or from some Fault in the Nutritive Processes in other Parts of the Body. We shall attempt briefly to analyse the pages devoted to this important practical subject; and shall commence with the disorders of digestion that result from defective action of the excreting organs. It is almost unnecessary to refer to the intimate relation subsisting between the stomach and the

* Digestion and its Derangements, pp. 374-5.

liver; if the secretion of the latter be defective from any cause, the functions of the former are almost invariably disordered; and there is a coated tongue, an impaired appetite, nausea, constipation, and often lowness of spirits, with disturbed sleep. For those who are not strong enough to bear the ordinary blue-pill and black-draught treatment, Dr. Budd recommends fifteen grains of bicarbonate of soda twice a-day, with enough of the potassio-tartrate of soda to act gently on the bowels.

Again, when the kidneys imperfectly discharge their office, the functions of the stomach soon become disordered. This is especially seen in the advanced stages of Bright's disease, where nausea or vomiting is often a prominent symptom. Hydrocyanic acid, conjoined (if there be undue acidity) with small doses of potash or soda, will generally check the gastric disturbance; or, if there be much vomiting, a minim of creosote in a bread pill may be administered three times a day before meals. If these means fail, Dr. Budd remarks that the vomiting and much of the associated gastric disorder may often be stopped for a time by a few purgative doses of cream of tartar and jalap, which probably relieve the stomach by causing a more abundant elimination of the noxious matter of the blood by the bowels. The medicine is best given before breakfast,

"Since it will then, in addition to the drain it causes from the mucous membrane, only sweep away the refuse of digestion, whereas if it be given at other times, or in repeated doses during the day, it sweeps away food that has been more or less digested, but the nutritious elements of which have not been absorbed." (pp. 248-49.)

But the blood may be rendered impure, and the stomach may be consequently disturbed, not only by the defective action of the great excreting organs, such as the liver and kidneys, but also from a fault in the processes concerned in the disintegration and elaboration of the tissues generally. One of the most important of these states of defective assimilation is that which is characterized by the formation and excretion of an excess of lithic (or uric) acid.

In persons who are in the habit of suffering from the derangement of the nutritive processes which is indicated by this symptom, indigestion is very common; it is chiefly marked by excessive acidity and heartburn, the urine at the same time generally depositing a sediment of what Dr. Budd terms lithate of ammonia, but what in reality is, for the most part, lithate (or urate) of soda. In these cases the bicarbonates of soda and potash are generally the best remedies, and the best time for giving them is two or three hours after the principal meals.

"Fifteen grains of either, two or three times a-day, is, in most cases, a sufficient dose; and if there be a sense of heat in the stomach, this may be conjoined, as Prout recommended, with a few grains of nitre. . . . . . The use of the alkalies should be continued for some weeks, and if any gouty symptoms exist, a grain of acetous extract of colchicum may be given at night." (pp. 251-2.)

The free action of the liver and of the bowels must at the same time be kept up by occasional small doses of blue pill, and by colocynth pill, either alone or in combination with extract of henbane. Medicine is however of little use in these cases, unless exercise, diet, &c., be duly attended to. The patient should take active exercise, be much in the open air, and should be restricted to a simple and abstemious diet—eat-

ing sparingly of animal food, and altogether avoiding rich dishes, pastry, cheese, &c.

The form of indigestion accompanied with the presence of oxalic acid in the urine, is next considered, but there is nothing requiring notice in his remarks on this subject.

The thirteenth lecture treats of "Forms of Indigestion characterized by some peculiarity in the symptoms—Urticaria—Pyrosis." Nettle-rash may be produced in various ways; but its most frequent cause, and that which especially concerns us at present, is the imperfect digestion of particular articles of food. Amongst the substances that have been observed to bring it on, are shell-fish, especially crabs and mussels, pork-pie, fish, when tainted or out of season, honey, mushrooms, cucumbers, almonds, and oatmeal. The symptoms are too well known to require notice. The main object of treatment is to expel as soon as possible the offending matter. The stomach should first be emptied by an emetic of ipecacuanha or sulphate of zinc, and the bowels then cleared by a warm but quickly-acting purge. To allay the cutaneous irritation, Dr. Budd is in the habit of prescribing a lotion, made by mixing half a drachm of acetate of lead and half an ounce of tincture of opium with eight ounces of water.

In those cases in which the nettle-rash seems to be referable to several substances in common use, rather than to one special substance, it may sometimes be kept off by the administration (before dinner) of the rhubarb and ipecacuanha pill mentioned in page 44, or of a few grains of rhubarb. Dr. Budd gives a case which shows very satisfactorily the occasional efficacy of rhubarb in this disorder.

"It sometimes happens (says our author), especially in women, that the nettle-rash, though depending immediately on the stomach, occurs only when digestion is weakened by over-fatigue, or by anxiety or some other mental emotion, or by profuse monthly discharges, and that remedies of a different class are availing. In some such cases, when all the means I have before spoken of had failed, I have known the eruption disappear under the use of carbonate of ammonia, alone or in conjunction with tincture of gentian." (pp. 271-2)

Dr. Budd enters at considerable length into the causes and nature of pyrosis or water-brash. For all practical purposes, we may regard it as dependent on two general causes—namely, (1) pregnancy, enlarged liver, or some other condition that disturbs the functions of the stomach; and (2) a defective diet, the fault most commonly being, that it consists too much of farinaceous substances. In the latter case, the treatment is chiefly of a dietetic nature, and little benefit can be expected from a purely medicinal treatment.

The medicines that have been found most useful in pyrosis, are (1) *astringents*, as bismuth, lime-water, kino, catechu, logwood, &c.; and (2) *sedatives*, especially opium and the salts of morphia. Medicines of these two classes may often be combined with advantage. As illustrations of such combinations, Dr. Budd mentions five grains of bismuth with a twelfth of a grain of the muriate of morphia, or five grains of the compound kino powder, or an efficient dose of catechu, krameria, or logwood, with opium, to be given before meals, two or three times daily.

Other medicines have at different times obtained a reputation for curing

pyrosis, which cannot be strictly classed either with astringents or sedatives; as, for instance, "nitrate of silver, which may be given in pills, in doses of half a grain, three times a-day; nux vomica, which may also be given in pills, in the dose of from three to five grains, three times a-day; quinine; and the mineral acids." (p. 282.)

When, as is often the case, the pyrosis is connected with anæmia, steel is of great service, both in removing it and preventing its recurrence.

Dr. Budd next describes "another kind of gastric disorder which is closely allied to water-brash, and which is characterized by paroxysms of violent pain in the stomach, often described as spasm, that comes on when the stomach is empty." (p. 282.) During the paroxysm the epigastrium is tender on pressure, the pulse becomes very slow, and the surface of the body cold. The pain is generally relieved by taking food and by lying down. The appetite in the intervals of pain is often good, and the digestion apparently normal; the tongue is usually pale and flabby, but clean. In most cases the patient is weak, and sleeps soundly.

This form of gastralgia seems to be induced in men by family cares, or anxiety in business, or over-fatigue, and is most frequent between the ages of thirty-five and fifty; in women, it is most frequently the effect of profuse monthly discharges. It may continue for two or three months, and is very likely to recur in persons who have once had it.

We extract the following notes, describing a case of this sort occurring in a medical man, aged between thirty-five and forty years:

"About three hours after each meal—or perhaps about four or five hours are nearer the mark—and as soon as digestion appears to be over, he is seized all at once with severe pain in the stomach, which soon becomes so intense as to depress the circulation in a very remarkable way. The pulse falls to thirty-five in a minute; he becomes deadly pale; his hands and feet grow cold; and all his strength appears to have left him. This state of things continues until he gets something to take in. But the very moment he swallows anything—and what is remarkable is, that the effect seems to be quite independent of the nature of the aliment,—he gets complete relief, until the period comes round when the stomach is again empty. In the absence of food, the pain is much assuaged by his assuming the recumbent posture. For the rest, his appetite is perfectly good; he does not suffer at all from thirst; and his bowels are regular as the day. The evacuations perfectly healthy in appearance. At times the stomach becomes enormously distended with wind, but this is not generally the case at the time of the paroxysm. He never vomits; never has nausea, water-brash, or heartburn. Sleeps well." (pp. 283-4.)

Since the time when the letter was written (the summer of 1846), the gentleman to whom it relates has had several illnesses of the same kind, most of them brought on by fatigue. Nothing is stated by Dr. Budd regarding the treatment in this particular case, but the medicine which he has found of most use in this disorder is hydrocyanic acid, given in full doses; opium is likewise of service, but generally less effectual than the hydrocyanic acid. The patient should live on a light but nutritious diet, have regular meals, and plenty of sleep; and should avoid fatigue, and all causes of excitement.

The next kind of gastric disorder we have to notice is what may be termed the indigestion of drunkards, often the combined result of drink and of exhaustion from want of proper food.

When it results solely from spirit drinking, its chief characters are—

"Want of appetite, and vomiting or dry retching in the morning, with a white or furred tongue, and a slow pulse. The power of digestion is much enfeebled, and if the patient eat at any time what for others would be a very moderate meal, he is apt to vomit soon afterwards, and to be troubled by pain in the stomach, and flatulence. . . . . This disorder, like the vice from which it springs, is most frequent in men of middle age, and is generally associated with more or less of that strange and peculiar disturbance of the nervous system which hard drinking brings on, and of which the most striking effects are inability to sleep, or sleep broken by frightful dreams, despondency in the morning, and tremulousness of the hands and tongue." (p. 286.)

This kind of disorder is very common among the poor of large towns, and sometimes is so severe as to lead to the suspicion that organic disease is present.

The most efficient remedies are bitters, opium, and solid food. Gentian, quassia, and calumba may be taken, singly or combined, in the form of tincture, two or three times a-day, an hour before the principal meals. With these bitters, small doses of opium or of morphia may be very advantageously combined, so as to tranquillize the nervous system, procure sleep, and settle and strengthen the stomach. It occasionally happens, however, where the gastric disorder is severe, that very large doses of opium are requisite. Dr. Budd describes such a case, in which no decided amendment took place during a four months' residence in King's College Hospital, till half a grain of muriate of morphia and five minims of dilute hydrocyanic acid were given every four hours.

"This soon stopped the vomiting, and gave him a full measure of sleep, and the appetite returned. He continued to take the morphia in very large doses, in conjunction with hydrocyanic acid; steadily grew stouter and stronger; and at the end of some weeks left the hospital to resume his accustomed labour." (p. 290.)

In all these cases it is essential that the patient should eat as soon as possible some solid nourishing food.

The lecture concludes with a brief reference to that comparatively rare form of gastric disorder in which digestion is tolerably good in the morning, so that a substantial breakfast causes no discomfort; while a hearty meal eaten in the after part of the day is followed by flatulence and gastric pain, which sometimes ends in vomiting. It is usually observed in old worn-out people. With regard to treatment, half a drachm of aromatic spirits of ammonia, or four grains of carbonate of ammonia, may be given three times a-day, and from four to ten grains of bismuth before dinner. Breakfast should of course be made the principal meal in these cases.

We may pass very rapidly over the last three lectures of Dr. Budd's volume. They are devoted to the "symptoms of stomach disorders—namely, pain and soreness of the epigastrium, vomiting, excessive acidity, and flatulence;" and to a notice of "some of the remedies for stomach disorders—namely, ipecacuanha, bismuth, the vegetable astringents, hydrocyanic acid, the alkalies, the mineral acids, the vegetable bitters, the preparations of steel, and purgatives." The last lecture concluding with a few pages containing "general rules of living." They are excellent lectures for students, but might have been omitted, or at all events much

abbreviated, in a work addressed to the medical practitioner. In his remarks on "excessive acidity," there are some statements of (we should think) very doubtful accuracy,—as, for instance, that the gastric glands secrete carbonic acid, and that oxalic acid may be generated in the stomach.*

The following observations on the vegetable astringents are valuable:

"Chalk, and the vegetable astringents,—kino, catechu, krameria, and logwood, —are generally given to restrain diarrhœa. It does not seem to be generally known that they are just as effectual, perhaps more effectual, in restraining undue secretion from the stomach. Chalk, like bismuth, from its sparing solubility, has little direct action, except on the mucous membrane over which it passes. The vegetable astringents have a more remote astringent influence. This is clearly seen in the colliquative stage of phthisis; where, besides restraining the diarrhœa and stopping the vomiting with increased secretion of gastric juice that often occurs in this state, they restrain, often in a very striking degree, the profuse sweating. They seem all to have much the same effect. I generally give the preference to krameria and logwood. Kino is not conveniently given in solution; and catechu is not only very nauseous, but, from being much used in the arts, is often of inferior quality. The most grateful to the taste is krameria; the most effectual, I believe, is logwood. Logwood has a mawkish taste, which is best corrected by cinnamon. An ounce of logwood shavings, and a drachm and a half of powdered cinnamon, may be infused for four hours in ten ounces of boiling water, and then strained. An ounce and a half of the strained infusion may be given two or three times a day, a short time before meals." (pp. 333–4.)

With regard to the employment of the alkalies he observes, that—

"It may, perhaps, be adopted as a maxim, that alkalies, given to exert their constitutional effect, are most frequently useful to persons who have dry skins and perspire little, and eat largely of animal food and live in towns; that acids are most frequently useful to persons who live in the country, eat largely of vegetable food, and perspire much. If there be one symptom more than another that suggests and justifies the use of soda, it is a furred or coated tongue." (p. 340.)

We shall conclude our notice of Dr. Budd's volume with an extract from his remarks on the vegetable and mineral tonics:

"The most important medicines of this class are the vegetable bitters—quinine, gentian, calumba, strychnine—and the different preparations of iron. Quinine, and the bitters generally, are especially grateful to persons who have injured their stomachs by hard drinking. With such persons they improve the appetite and strengthen digestion, and have a bracing effect upon the system at large. In persons exhausted by over-work, or wherever weakness of the stomach is the result of general debility from other causes, they often do much good in the same way—*by improving the appetite* and strengthening digestion. They do harm in organic diseases of the stomach; in plethoric states of the system; and generally where there is a furred tongue, or where the urine throws down a sediment of lithic acid or of lithate of ammonia. Their most striking effect is, to improve the appetite, when this has been impaired from hard drinking, or from over-work, or from nervous exhaustion from other causes; and the best time for giving them is from half an hour to an hour before meals. The different bitters have not precisely the same effect. Calumba has a sedative influence not possessed by the others, and probably on this account has had a wider reputation as a remedy for mere indigestion. Gentian and chiretta (which is of the gentian tribe, and is much employed by practitioners in India) tend to increase the secretion of the liver, or at any rate do not impede its secretion, which quinine and quassia seem often to do. They

* See p. 306.

are therefore better suited to bilious persons, and to those cases of indigestion where the secretions of the liver are defective. The different preparations of steel are especially useful in the indigestion that occurs in chlorosis, and generally where weakness of the stomach results from anæmia. They do harm in plethoric states of the system, and generally where there is a furred tongue, or where the urine throws down a sediment of lithate of ammonia or of lithic acid. The citrate, or ammonio-citrate, is the most agreeable preparation to the taste, and generally the most grateful to the stomach. If there be any disposition to sickness or nausea, or any tendency to furring of the tongue, it may be given in conjunction with the bicarbonate of soda or potash. This makes a mixture having much the same effect as Griffiths' mixture—the mistura ferri composita,—and far more agreeable. The muriated tincture of iron is more astringent than the other preparations, and may be given in conjunction with dilute muriatic acid, in the forms of indigestion suited to this latter medicine, when these exist in states of anæmia. The sulphate of iron, like the other metallic sulphates, has a tendency to cause sickness, and should not be given in cases where a disposition to sickness exists. Steel medicines do good by improving the quality of the blood rather than by their immediate action on the coats of the stomach, and are best given at meal-times. They then are mixed with the food, and gradually absorbed with the products of digestion, and are less apt to offend the stomach and to cause headache than at other times. Whenever steel medicines are given, it is essential that a regular action of the bowels be kept up. These medicines tend to confine the bowels and to cause evolution of sulphuretted hydrogen in them; and, unless this tendency be counteracted, they are apt to fur the tongue and cause headache." (pp. 343-5.)

We now turn to the concluding hundred pages of Dr. Chambers's volume, which treat of subjects to which Dr. Budd has hardly at all adverted, and we shall commence with the consideration of the "Morbid States of the Small Intestines interfering with Digestion."

Dr. Chambers first considers those abnormal states which exhibit a *deficiency of intestinal absorption*, and then those in which there is *defective excretion*.

Defective absorption may occur (1) in general disease—as, for instance, in the later stages of continued fever; (2) in cases of chronic ulceration, which especially occurs in tubercular persons; (3) in mucous flux of the intestines; and (4) in intestinal struma. We shall briefly notice each of these conditions, especially in reference to treatment; and commence with "the rational and physiological view of the treatment of acute fevers through the digestive organs."

"In these cases (says our author) there is either introduced or generated, or both introduced and generated, in the body a poisonous substance foreign to its tissues; and it seems most proper that this should be evacuated before there is introduced any more fresh matter than is absolutely necessary to keep life up. . . . . For this reason, in acute fevers it is of the highest importance to watch for the time when destructive metamorphosis begins to return. Up to that moment any food we give with a really nutritive intention is either useless or noxious; and any complementary food which arrests metamorphosis is [causes?] a postponement of the favourable turn of the disease. Decoctions of starch, infusions of gum, and fluid gelatinous drinks, are possibly beneficial, by shielding the mucous membrane from irritating substances; but they are not likely to be absorbed, and their chief use is as a placebo to the patient and the friends, who dread starvation.

"How far we should interfere to *promote* destruction, by giving neutral salts, water, purgatives, mercurials, &c., must be a question to be decided by the peculiarities of each individual case. . . . . In almost every instance I am inclined to

think some of these agents beneficial; and among them I would call the attention of practical men to one undeservedly neglected in the present day—viz., the administration of emetics at the onset of fevers. I am sure that by this powerful agent the first period of the fever—the period before the destructive metamorphosis commences—is much shortened, and the subsequent violence of the phenomena abated. The secretions of the alimentary canal, arrested in the mucous membrane, and subjected to chemical decay there, are cleared away, and whatever poisonous or noxious matters may have been received into the hollow viscera, are removed. Sometimes the recovery of destructive metamorphosis follows immediately on the remedy, and is therefore so slight that the attack is said to be cut short; but more commonly only a temporary stimulation of the vital powers occurs, and the natural latter part of the course of phenomena is gone through.

"The recommencement of destruction is announced by the increased amount of solid matter in the fæces, or the increased specific gravity of the urine or perspiration, with the simultaneous alteration of countenance by the falling in of the features, and other evidences of commencing emaciation. This is the time to let a continuous stream of digestible nutriment begin to flow through the alimentary canal, to be taken up at the auspicious moment when the absorbents are to receive it."*

In his remarks upon the treatment in cases of chronic ulceration of the small intestine, Dr. Chambers lays great stress upon the danger which patients threatened with tubercle before the age of thirty years† run in removing to warm climates—as, for instance, Madeira, the Azores, or Cairo. The risk of injuries to the bowels from the diarrhœa so often induced by the change of food and climate, counterbalances the chance of good for the lungs. Sulphate of copper, in combination with opium, is, in his experience, the most powerful means of checking the diarrhœa which so often occurs in tubercular patients with ulceration of the small intestines.

Our limited space precludes us from noticing his excellent description of the symptoms of "intestinal mucous flux." Children seem more liable to this affection, as well as to the worms which so frequently accompany it, than adults, and females than males.

"The causes of this disorder are usually to be traced to continued low temperature, united to damp, or rather to a want of the occasional variation of a warm dry air; sedentary habits, especially when united to defective air; and in the upper classes, anxiety and intense occupation of mind in study, by impeding proper digestion of the food, are frequently joint causes. And when once the disorder has commenced, there is no more powerful aggravator of it than continued mental exertion on one subject." (p. 461.)

In the treatment of this affection, our author lays far more stress on hygienic measures than on purely medicinal agents.

The bowels having been well cleared out by a free dose of oil of turpentine, or, if the patients rebel at the turpentine, by a few doses of calomel and jalap, we must, if the circumstances of the invalid allow of it, recommend travelling, or, if a complete absence from home cannot be

* Chambers, op. cit., pp. 454–5.

† Dr. Chambers shows, in his Decennium Pathologicum, that the per-centage of those attacked by ulceration of the small intestines in connexion with tuberculosis of the lungs, in the fatal cases at St. George's Hospital, was

from 15 to 30	years of age	33·6 per cent.
„ 30 to 45	„	23·8 „
„ 45 to 60	„	18·0 „

obtained, riding on horseback. We cordially agree with Dr. Chambers in thinking that

"No directions for a tour can be so judicious in these cases as the picturesque parts of our own land: Cumberland, Wales, Devonshire, and Cornwall afford invaluable opportunities for a mixed saunter in carriage, on foot, and on horseback, according to strength or inclination, without the risk of foreign diet." (p. 466.)

His observations on the importance of horse exercise, and of a due amount of sleep, in this disease, are well deserving of the best attention:

"A rapid change in the appearance and smell of the fæces follows the use of *riding*, and that is soon succeeded by restored health and strength. The gentle shaking motion probably induces more active secretion in the liver, and absorption in the intestines at the same time, by hastening the circulation; for the fœtor of putridity, which it is the business of the bile to prevent, and the undissolved muscular fibre, which it is the business of the bowels to take up, disappear simultaneously from the stools. Though with bad external piles it is very inconvenient to ride, yet in mild cases very great relief is often obtained, and I do not think that hæmorrhoidal tumours should ever contra-indicate at least a trial of horse exercise. The value of *sleep* is sadly under-estimated in chronic cases by medical men. . . . There is probably no disorder in which this is so important as in mucous flux of the intestines, and I have known the expedient of lying in bed till ten o'clock in the morning make treatment effective which previously had been perfectly useless." (p. 467.)

We shall give, in as condensed a form as possible, our author's views on the treatment of intestinal struma as it occurs in children, and the strumous dyspepsia of adults. The two great points to be considered are (1) to supply such a diet as is most capable of being absorbed under the circumstances; and (2) to increase the activity of the vital functions by a combination of those medicines which promote growth, and those which promote destruction.

(1) A nutritious animal diet, given frequently, in small quantities at a time, may be commenced with advantage at any period of the disease, and persevered in throughout. Broths and flesh-teas, slowly cooked at a low heat, with the addition of a few drops of hydrochloric acid, answer well when solid meat excites nausea, although they are probably not more digestible than good old mutton, plainly cooked. "Nothing takes a child's fancy so much as small birds—a lark or a blackbird will often be eaten with pleasure when other things excite disgust; and (perhaps from the gratification of imagination) is easily digested." Milk is seldom borne well in its natural state. The best way is to partially skim it, add one-third part of lime-water, and some sugar; it then forms an excellent ordinary drink to be taken at meals. Soda-water and cream, mixed in the proportions most agreeable to the patient, form a good draught to be taken the first thing in the morning. Bread and potatoes constitute the best vegetable food. If, in the case of adults, any alcoholic drink is required, that which, according to our author's experience, usually agrees best "is Bordeaux wine, of recent vintage, so as to be sound, free from acidity, and with the agreeable roughness of the fresh grape still remaining, to act as a tonic to the mucous membranes." (p. 474.) Burgundy and port stand next in value; sherry, Madeira, and malt liquors do not usually agree, and distilled spirits are still more objectionable.

"In this state of the system," says Dr. Chambers, "sugar will often be found a very digestible, and when digestible a very useful, article of diet, especially for children. Its addition to cows' milk produces a nearer resemblance to the natural nutriment of the young of our species than can otherwise be made. Spread on bread, it is usually relished much; and given quite alone, in syrup, toffee, and the like, often increases the appetite. Whether it acts as a complementary food or merely as an accessory, or both, it certainly augments the quantity of matters assimilated by the body under the circumstances of the disease now spoken of. I have had little patients, affected with strumous derangement of digestion, increase in weight to the extent of from two and a half to four pounds in the first week after commencing the use of an additional four ounces* of sugar to the daily diet.

"Where there is joined to strumous mal-digestion a tendency to disease of the osseous system, to rickets, or softened bones, sugar is particularly indicated by its power of arresting the metamorphosis of the bones;† and Dr. Böcker has found great advantage from joining to it an additional quantity of phosphate of lime to that which is ordinarily found in a bread-and-meat diet." (p. 475.)

2. First amongst the drugs, Dr. Chambers places cod-liver oil; and he prescribes it in a different manner from that in which it is usually ordered.

"I usually," he observes, "order hours for taking the oil to be selected as far as possible removed from the ordinary meal-time. I find also that it is a good plan to make the dose still more of a separate meal by washing it down with a wineglass of half-and-half milk and lime-water, or milk and soda-water, with a mouthful of biscuit, so as to neutralize any rancidity in the oil, and add some more matter for absorption." (p. 476.)

We have ourselves prescribed this medicine very largely, and in almost every possible manner, ever since Dr. Bennett published his treatise upon it (in 1841); and we have certainly found that, as a general rule, it causes far less inconvenience to the patient when taken immediately after meals than on a comparatively empty stomach.

Steel is far preferable to vegetable tonics; and the finely-levigated *fer porphyrisé* of the French Pharmacopœia is especially recommended:

"At the same time, with these medicines, given with the design of increasing the building and formative functions, there should be administered those which augment the corresponding powers that make up life between them—the increase of metamorphosis. A full allowance of water and salt in the diet is desirable; and sea-bathing is beneficial on the same ground." (p. 477.)

Iodine—we presume Dr. Chambers means iodide of potassium—is also useful as an increaser of metamorphosis; the most powerful, however, of these agents is mercury, which must be prescribed very cautiously, lest it outruns the tonics we are administering, and causes destruction to exceed growth. The form in which he recommends that it should be given is hydrargyrum cum cretâ, in one or two-grain doses, with double the quantity of sesquicarbonate of soda, and a grain of powdered rhubarb; and its use should be suspended as soon as the fæces present a normal appearance.

We now arrive at the second portion of this chapter, in which the

* "A quantity larger than is necessary or desirable, but given in the instances above mentioned for the sake of trial."

† The experiments of Dr. Böcker (Beiträge zur Heilkunde, vol. i. p. 38) distinctly show that sugar has a greater power than any other known substance in restricting the waste of the body by decomposition, and that its effects are most marked on the products of the decomposition of bone, the earthy phosphates in the twenty-four hours' urine being lessened by more than one-half when sugar is taken. See the Physiological Part of Dr. Chambers's volume, pp. 240–5.

subject of *defective excretion* is discussed. Dr. Chambers lays down very distinctly the difference in the symptoms produced by a defective secretion or discharge of bile, and a general defective excretion from the intestines, which we are now considering. In local disease of the liver or gall-bladder, there is jaundice of the external surface, accompanied by clay-coloured stools, whenever the flow of bile is impeded, and no change of colour so long as it is free. In general defective excretion from the intestines, there is a dinginess and darkness of complexion, but none of the yellow-green of jaundice, and the stools are scanty but not clay-coloured.

"In every case of arrested intestinal excretion, the bowels are costive and the stools remarkably scanty—usually dark, hard, and dry; the urine from time to time deposits copiously lithate of ammonia, and is often dark-coloured, but not otherwise unnatural. Loss of excretive power in the intestines in some cases leads to a remarkable form of paralysis. The muscles of the lower extremities gradually, by almost imperceptible degrees in general, lose the power of progression. It is not so much that they cannot move, but that they move with excessive slowness; and they are not, as in ordinary paralysis, incapable of sustaining the body in the upright position: the patient can stand pretty well, but cannot go. There is not, moreover, any deficiency of sensation. Sometimes the derangements of digestion, gastric and intestinal, which had preceded this paralysis, vanish on its supervention, but this is not always the case. This partial paralysis is most frequently observed in Europeans returned from an Indian climate, and, I have understood, is by no means uncommon in Mexico and South America. But I have seen it in those who had never left England; and its greater frequency in the victims of temperature is probably simply occasioned by the greater frequency of its true cause—viz., the loss of intestinal digestion in those parties." (pp. 481-82.)

How are we to determine that this loss of power in the lower extremities is not dependent on lesion of the spinal marrow? It is unnecessary to say that this is a point of the greatest importance, because the affection of which we are now speaking is usually amenable to treatment, while the spinal disorders with which it may be confounded are almost always incurable.

"The most marked difference between the two lies in their history. This form of paraplegia comes on exceedingly gradually, so that it is very difficult for the patient to date, not only the commencement of the disorder, but any period at which it has grown worse; while spinal disease has, if not a sudden beginning, yet always sudden aggravations from time to time, and after each aggravation the patient is decidedly worse than at any previous day. The history also, and the present symptoms of deficient intestinal excretion, can guide the practitioner to a diagnosis." (p. 483.)

The treatment of defective excretion from the small intestines is discussed with greater brevity than we could desire. The application of leeches to the anus is of much service in this affection.

"Two or three leeches at night, followed by a tepid sit-bath, should be the maximum dose; but it may be repeated every night or every other night for a fortnight or three weeks together, if the general strength and the making of wholesome blood be at the same time promoted by an easily digestible diet. It is remarkable how the freshness of complexion and the transparency of the skin will gradually return, and often weight be gained, during the employment of this practice." (p. 485.)

The value of hydropathy in deficient intestinal excretion is then alluded to. Our author regards it as a very doubtful remedy, and would

only recommend it in very special cases, and then with great caution; in some cases it has seemed to bring on the paralysis to which we just now referred. The activity of metamorphosis is more safely augmented by the judicious use of mineral waters. "A saline, at first alone, and then mixed with a chalybeate, both of moderate strength, accomplishes the purpose." Waters containing sulphureous gases should be avoided in these cases.

There is a chapter on "Flatulence," from which, if our space had permitted it, we should have made two or three extracts. We must confine ourselves to his remarks on the treatment of *fœtid flatus.*

"Flatus, of which the smell is not that of healthy fæces, but of decomposed organic matter, indicates that the duty of the physician lies in restoring some decided deficiency in the process of digestion. Most commonly it is the liver and small intestines which are wanting in activity, so that the stools are scanty, slimy, and irregular. The most effectual remedy is mercury, in small doses, combined with light vegetable tonics—such, for instance, as a nightly powder of hydrargyrum cum cretâ, and a dose of decoction of sarsaparilla, or infusion of gentian, or of red bark, thrice a-day. The quantity of unabsorbable woody fibre which is contained in these vegetable medicines appears to be an advantage, as it carries the active portion of the drug lower down the alimentary canal, and perhaps also is useful in forming a more bulky fæculent mass. The dietary should be sparing, and all difficultly soluble albuminous articles should be avoided: such, for instance, as solid white of egg, or meat pastry. Warning also should be especially given against food in a state of half decomposition, such as game long kept, stale fish, ripe cheese, medlars, sour beer, half-fermented champagne, &c. These not only decompose themselves, but cause all that surrounds them to decompose too, turning wholesome victuals into noxious poison." (p. 506.)

"Digestion and its Derangements" is one of the most interesting and instructive works of its kind that we have read; and we believe that Dr. Chambers has done good service to medical science in showing, as he has done, the intimate relation which exists between physiology and practical medicine. We have little doubt that a second edition will soon be called for; and we would suggest that the author might then advantageously omit certain portions of the chapters on the "Physiological Action of Substances submitted to Absorption in the Alimentary Canal," and on "Regimen," and reproduce them in an enlarged form as a "Treatise on Diet and Regimen." None of the existing English works on this subject approximate to the present state of our physiological and chemical knowledge; and we know of no one better qualified than Dr. Chambers to undertake such a task. A well-written scientific book on this subject would be equally serviceable to the practitioner and to the patient.

To those of our readers who wish to make themselves thoroughly acquainted with the anatomy and physiology of the stomach and intestinal canal, we can cordially recommend Dr. Brinton's Article in the Cyclopædia of Anatomy and Physiology. The only reason why we have not noticed it more fully in the preceding pages, is, that we wished to confine ourselves closely in this article to the diagnosis and treatment of gastric and intestinal disorders.

G. E. Day.

Review III.

Researches on Colour-Blindness. By George Wilson, M.D., F.R.S.E., Regius Professor of Technology in the University of Edinburgh.—*Edinburgh*, 1855. pp. 180.

Since the publication in 1794, by Dalton, of his own case, the occasional occurrence of the peculiarity of vision called "colour-blindness," by Dr. Wilson, has been well known to the world.* It has been regarded as a curious phenomenon, worthy of being made the subject of interesting inquiry and of amusing curiosity, even by those who experience it in their own persons, but not of any practical importance. It is true enough that those educated classes to whom the phenomenon is likely to be familiar are rarely placed in positions where their capability of distinguishing colours is of much importance to themselves or others; for a superior intellect finds no difficulty in supplying the want of acuteness of external sense, so that the deficiency is scarcely felt. It made but little difference to such men as Dugald Stewart, Dalton, or Sismondi, to be ignorant whether the ink by which they conferred imperishable inheritances on their race was red, green, or black; the same deficiency would not interfere with the legal acumen of the barrister, or the successful practice of several medical men quoted by Dr. Wilson. But our author has the merit of being the first to call attention to the fact that, in several of the lower walks of life, this bodily imperfection not only incapacitates the individual from following his calling with success, but sometimes may imperil the lives of others as well as his own. Not only does the weaver, the tailor, the gardener, thus affected, commit mistakes, ludicrous enough to the bystanders, yet likely to lead to the starvation of the poor man who by them loses his work; but the railway guard or pointsman, and the sailor, especially in steam service, may, by misunderstanding coloured signals, cause the death of thousands. We think, therefore, much gratitude is due to the Professor of Technology for thus calling attention to the subject, and investigating it with the industry exhibited in the volume now reviewed.

Three forms, or rather degrees, of colour-blindness are distinguished by the author.

1. Inability to distinguish between the nicer shades of composite colours, such as browns, greys, and neutral tints.

2. Inability to distinguish between the primary colours, red, blue, and yellow; or between these and the secondary and tertiary colours, such as green, purple, orange, and brown.

3. Inability to discern any colour, properly so-called, so that black and white (i. e., light and shade) are the only variations of tint perceived.

The *first*, or lowest, degree is *apparently* the rule rather than the exception among persons of the male sex, whose eye has been as little educated as is usual in our unæsthetic community. Dr. Wilson found that of sixty persons attending the Chemical Class of the Edinburgh Veterinary College,

* Before that essay, only occasional notices of the phenomenon had occurred. These are collected by Wartmann, a translation of whose paper may be found in Taylor's Scientific Memoirs for 1846, p. 162.

the great majority declined to give names to any colours but red, blue, yellow, green, and brown. Without care, one might easily be led into thinking colour-blindness to be much more common than it really is. But where the subject has so seldom entered the mind, and where the nomenclature consequently is so defective, it is better to dispense altogether with the *names* of colours in testing cases of this affection, and to set the task of arranging pieces of cloth or skeins of worsted in such a way that the same colours and shades should be classed together. When this is done, it will frequently be found that those who make no mistake in matching full tints of the primary and secondary colours, err in certain of the fainter shades of both, and in all the shades of some of the more mixed colours. Thus the difference between pink and pale blue is a puzzle to many who do not otherwise confound colours.

"Mr. Crombie, dyer, Brown-street, Edinburgh, informs me of three persons known to him, connected with dyeing, to whom the tints in question were a constant source of mistake. Messrs. Grieve, late of South Bridge, had in their employ a person who could match all colours but drabs. Professor S. is never certain, even by daylight, of the difference between blue and green; and many persons confound pink with pale yellow."

Where this slight degree of colour-blindness is congenital, it is just as incurable as the more marked forms. But still it is practically convenient to separate it from them, because up to this point it may be simulated by a want of discrimination, which is simply the result of deficient education, and which, therefore, attention is quite capable of removing.

The *second* form is that which Dr. Wilson has most investigated, and has cited in detail a great number of marked cases. In it, red, blue, yellow, purple, orange, green, brown, &c., are respectively mistaken for one another, or confounded together. In less severe cases, the majority of colours are seen accurately, but two at least (as red and green), and generally four (as red, green, olive, and brown), are not distinguished from each other.

There is a considerable difference between the colours as regards their liability to be mistaken for one another; and curiously enough, it is those which to ordinary eyes appear most violently contrasted, that are confused together, whilst tamer and less vivid tints are correctly appreciated. Thus, while yellow is almost universally recognised, and rarely mistaken for its complementary purple, red, however glaring, is constantly confounded with green, very often with black, and for some appears to have absolutely no existence. Blue, when pure, is usually detected pretty readily, and also yellow, as above mentioned; yet their combination, green, is one of the greatest stumbling-blocks to the colour-blind.

The *third* variety, where all objects are to the eye mere light and shade, and where absolutely no difference between colours can be detected, is extremely rare, and no instance appears to have come under the observation of the author. One, however, which he describes, is a near approach —viz., of the physician (Case VIII.), who confounds *all* colours equally by daylight, though by gaslight and transmitted light he is able to sort them rightly.

The disadvantages of this defect, even in its slightest degree, we hold to be something positive, and worthy of consideration. It is surely a mis-

fortune to miss one of the purest and seldomest-abused pleasures which God has given us: but independently of that, it obviously renders a man less fit for a great number of positions in life to which inclination and circumstances might otherwise lead him. Mourning warehouses are, doubtless, a great resource for the unfortunate haberdasher who matches red with black—but what is to happen to the house-painter accustomed to have his colours mixed by his wife, when he becomes a widower? We have ourselves interceded for a country carpenter who painted half a railing leaden-grey to match a green, and saved him from losing his job as well as his paint; and how a barrister would annihilate the medical witness who could not distinguish an arsenical precipitate by its tint! Dr. Wilson's stories of the tailor who sewed a black coat with red thread —of the chemical lecturer who always had to ask, instead of telling, his pupils the coloured reactions in his experiments—of the physician to whom *scarlet* fever had no existence—of the gentleman who condoled with a female friend dressed in vivid green, supposing her to be in mourning—of the Quaker who bought a bottle-green coat for himself and some scarlet merino for his wife—of the school-girl attempting to arrange the colours in her drawing by the taste, &c., &c., are striking and amusing instances of the difficulties consequent on this defect. One wonders how he managed to come across so many; but that is explained by his having advertised in the 'Athenæum' the fact of his being engaged in these researches, and obtaining in answer the details of their cases from the sufferers themselves.

But of more importance is the consideration that the use of colour in railway and naval signals sometimes places the lives of hundreds on the chance of the interpreter having as full a conception of the chromatic tints, as the captain or the board of directors;[4] and the first discovery of the defect may be at a grim inquest which it has given rise to.

In the Admiralty notice respecting lights to be carried by sea-going vessels, to prevent collision, which is at present in force, the order is that all British steamers are to show, "between sunset and sunrise, a *white* light at the foremast head, a *green* light on the starboard side, and a *red* light on the port side." When, then, one vessel is crossing another's bows, the foremast light goes for nothing, and the steersman of the latter is expected to know by the colour which side of the crossing vessel is towards him, and consequently if it is going to his right or his left—and on a dark night has no other guide whether he should port or starboard his helm. A similar principle is applied, both by night and day, in railway signals.

It may be suggested that it is not likely that an individual should be so long ignorant of the partial want of a sense, and should not be aware of its absence, till called upon in adult life to exercise it. But Dr. Wilson gives several instances of this obtuseness of their own fault in the colour-blind: one especially of a tailor, who never discovered it till he was promoted to be foreman, and had to match cloth for the journeymen, when he distinguished himself by providing a scarlet livery with green strings, and informing a customer that a red and blue stripe was all blue. He had, of course, worked at his trade for many years before being raised to his responsible post, and was, in fact, known for his excellence as a cutter.

It may also be stated, that no case of accident on rail, road, or water, has as yet come before a court of justice, and therefore that the fear of it is a phantom wall. Possibly—but possibly, also, the phenomena may, if thought of and investigated, have explained the strange obtuseness and apparent negligence in officials which has often puzzled juries.

These many inconveniences are not, however, without some advantages to weigh against them. The deficiency in the sense of *Colour* appears to allow fuller scope to the due appreciation of *Form*. In point of fact, even the most healthy eye can seldom take in both these ideas in perfection at the same moment. The late Mr. Dewint, than whom none has contributed more to the love of external nature in our generation, used to teach his pupils that, for the full understanding of any objects, two drawings were necessary—one showing the colours, and the other the form; and that this arose as much from the mental constitution in the beholder, as from the imperfect skill of the artist. The visitors to the Universal Exhibition of Pictures at Paris last year, who enjoyed the unique opportunity of comparing in masses the æsthetic perceptions of each nation, could not fail to remark how the harmonious luxury of colouring in the British gallery was united to drawing very inferior to its neighbours; and how the unequalled delineation of form by the French school with difficulty removed the sense of pain occasioned by chromatic errors. To pass rapidly from either to the other gave a most unpleasant jar to the feelings. It is to be expected, therefore, where there is the extraordinary partial deficiency of the sense of vision which we are considering, that a compensation should exist in the greater acuteness of other parts. And such seems to be the fact. Numerous proofs are to be found in Dr. Wilson's pages. Among the colour-blind, Case III. was the *prize student* of his class as a drawer from nature. Case IX. says, "I find, at times, many of my brother engravers in doubt how to translate certain colours of pictures, which to me are matters of decided certainty and ease. *Thus to me it is valuable.*"

Mr. N., of Torquay (Case X.), thinks his *colour-blindness an advantage to him when engaged as an amateur artist.* "In crayon-drawing I believe I have, in consequence of this defect, a more just appreciation of light and shadow, and the value of chiaro-oscuro in composition." It is remarked, also, that the tailor above quoted keeps his place as foreman, in consequence of his excellence as a cutter, in which occupation of course an eye for form and outline is the prime requisite.

The number of the colour-blind is larger than has been generally supposed. From a table exhibiting the results of the examination of 1154 persons of various classes, at Edinburgh, in 1852–53, it may be inferred that in the Northern metropolis 5·6 per cent. (or more than 1 in 18) are thus defective. Of this 5·6 per cent., 1·8 per cent. (or 1 in 55) confound red with green; 1·6 per cent. (or 1 in 60) confound brown with green; 2·2 per cent. (or 1 in 46) confound blue with green. Dr. Wilson remarks that the distribution of these numbers among different classes of the population is "most capricious," and makes no attempt to trace it as peculiar to any. We would, however, call his attention, in future researches, to one strange fact appearing on the face of his tables—viz., that the very highest per-centages are among those whose circumstances

may be supposed to have best enabled them to overcome natural defects. The largest is among the medical attendants at the Royal Asylum, Morningside; and the next, his own pupils. This proves at least that education is not likely to do much for its removal. That a high appreciation of colour may come without teaching, is also evidenced by a young Caffre, who "named the colours shown him with great rapidity and precision, although his English vocabulary was necessarily limited; and matched Berlin wools and tinted papers with a readiness and unhesitatingness, such as even practised professional colourists might envy." The sacrifice of the human form divine to a love for personal chromatic decoration, as exhibited by some of Dr. Latham's ugly friends at the Crystal Palace, and confirmed by all admirers of savagery, would seem to associate a taste for colour, or at all events the surrender of form for the sake of colour, with an absence of civilization.

As to the influence of sex, Dr. Wilson's researches, both the earlier series included in the body of the work, and latter contained in the appendix, show that colour-blindness is much rarer among females than males. He does not, however, give numbers, but proposes the subject as one open to inquiry by teachers of schools, medical inspectors of factories, &c.

The thirty-four pages devoted by the author to the discussion of the theories of this defect are inconclusive, and, from the very fact of their inconclusiveness, will not bear condensation. All that he deems himself entitled to affirm is, that in the colour-blind "the cerebro-retinal apparatus of vision is unendowed with that sensitiveness to colorific impressions which it possesses in those whose vision is normal;" that is to say, that the cause lies between the humours of the eye-ball and the mind; excluding both the suggestion of Dalton, of its being due to the existence of an abnormal colouring in the lens or vitreous humour, and the idea which might not unnaturally occur to a metaphysical theorist, that the defect was not in the body at all, but in the mind. Dr. Wilson has spent several pages in overthrowing the first-mentioned explanations; and it may be observed that his trouble is not without considerable practical importance, for an Italian oculist, Dr. Trinchinetti, has actually proposed the extraction of the lens as a cure. To stop him, if possible, from carrying his design into execution, a case is recorded of a gentleman who had both lenses removed for cataract, and experienced afterwards, on many occasions, temporary attacks of colour-blindness, which before the operation never occurred.

The great majority of cases of permanent colour-blindness are congenital, and their cause is to seek only in the theories above discussed; but instances of it are related by Mackenzie, White Cooper, and others, where it could be traced to "congestion, hepatic derangement, and dyspepsia."* The disorder is usually temporary under such circumstances, and disappears after the general treatment employed for the state of health. A curious example is, however, detailed among Dr. Wilson's cases, where it was caused by a severe accident, attended by concussion of the brain and long-continued cerebral excitement. Here the colour-

* Article, Vision; Cyclopædia of Anatomy and Physiology.

blindness was permanent, and assumed all the intensity and usual symptoms of the congenital origin.

The cure of colour-blindness by any physical agents seems hopeless; and, according to Dr. Wilson's experience, education seems to do but very little towards removing the defect from the body or the mind, whichever it is situated in. No case is on record among the many highly-educated persons who have thus suffered, of anybody inventing a means of improving their power of chromatic diagnosis.

There are, however, some methods of alleviating or correcting the false judgments which the imperfect sense is led into. One is the *comparison of doubtful with known colours*, by carrying about a chromatic scale, accurately tinted and named. This, however, is available only to a limited extent—that is, as far as the colours of the scale itself can be distinguished. Another is the *employment of touch*, to distinguish the alterations in texture made by different dye-stuffs. This may be of some use to clothiers, weavers, &c., who have a limited number of goods to arrange, but would not assist a customer in purchasing unknown articles. Perhaps to painters *the sense of smell*, higher educated than is usual, might be a help, and would not be open to the obvious objection lying against the employment of the taste.

A more important suggestion, which we owe to Dr. Wilson, is the *substitution of artificial light for solar* in the examination of colours. It certainly is a strange thing that candlelight, which to the healthy eye causes a confusion of delicate tints, should render them more easily distinguishable by a morbid sense. Such, however, is the case, as several examples quoted by Dr. Wilson are sufficient to show. The most striking of these is that of a draper, who has long been in the habit of keeping a gaslight burning in a dark room, to enable him by day to distinguish scarlet from green, and crimson from blue. This is quite sufficient to induce every colour-blind person at least to try the effect of artificial light in correcting his erring perceptions.

From the observation, that it is the yellow colour of ordinary artificial illumination which assists the colour-blind—very white light, such as the lime-ball and electric charcoal light, being as useless as the solar rays—Dr. Wilson inferred that the *employment of yellow or orange transparent media* might be of advantage; and such has proved to be the case in several trials. It is, however, as might be expected, an inferior expedient to the direct use of yellow illumination; and the question of the best colouring substance, whether silver, uranium, iron, or organic matter, is still open to experiment in each individual case.

The employments for which colour-blindness most seriously disqualifies, are those of sailor and railway servant, who may daily peril human life on an indication which a coloured flag or lamp is intended to give. For this evil there are two remedies. First, a careful examination of the parties employed, to test their capability of distinguishing rapidly the signals used; and secondly, to make the shape and movement of signals the chief index of their meaning, so as to dispense with colour except as a subordinate aid to this. It is gratifying to learn that the Great Northern Railway Company have, apparently in consequence of Dr. Wilson's observations, directed that in future all their porters shall be tested

as to their freedom from colour-blindness; and it may fairly be expected that other lines will follow the example. We do not know whether it is in consequence of the observation already quoted, of the rarity of colour-blindness among women, or because they require less wages, that they are extensively employed as signalers on several of the French railways (the Havre, and the Paris and Lyons lines, for example). The other and more effectual mode of prevention, the substitution of form for colour in signals, we fear, has not been taken in hand by any of the managing bodies to whom our lives and liberties are so freely entrusted; and we would call serious attention to the special remarks which Dr. Wilson has made to this subject in the Supplement to his interesting little volume.

Review IV.

1. *On the Disorders of Infantile Development, and Rickets; preceded by Observations on the Nature, Peculiar Influence, and Modifying Agencies of Temperaments.* By A. Schoepf-Merei, M.D., Lecturer on the Diseases of Children at the Chatham-street School of Medicine, Manchester, formerly Chief Physician to the Children's Hospital of Pesth, &c.—*London,* 1855. pp. 218.

2. *An Analysis of* 3000 *Cases of various kinds of Deformities admitted at the Royal Orthopædic Hospital, Bloomsbury-square.* By Edward F. Lonsdale, Surgeon to the Hospital. ('Lancet,' Sept. 1855.)

3. *Lettsomian Lectures: Diseases of the Bones.* By John Bishop, F.R.S. ('Lancet,' Oct. 1854.) *On Deformities,* &c. ('Lancet,' April, 1848.)

4. *Ueber Rachitis.* ('Journal für Kinderkrankheiten,' verschiedene Bände, besonders Bände xvi., xvii., von Trousseau; xvii., von Hauner; xx., von Vogel; xxii., von Stiebel.

On Rickets. ('Journal for Diseases of Children,' vols. xvi., xvii., by Trousseau; xvii. by Hauner; xx., by Vogel; xxii., by Stiebel.

5. *Zur Kenntniss der Periostitis Infantum (Rhachitis).* Mittheilung von Prof. Hermann Meyer, in Zürich. ('Zeitschrift für Rationelle Medicin,' von Dr. Henle & Pfeufer, Bänden iii., vi., neue Folge.)

Contributions to the Knowledge of the Periostitis of Infants (Rhachitis). By Professor Hermann Meyer. ('Henle & Pfeufer's Journal,' vols. iii., vi., new series.)

6. *Das Normale Knochenwachsthum und die Rhachitische Störung desselben.* Von R. Virchow. ('Archiv für Pathol. Anatomie,' Band v. Heft 4, § 409. Analysed and condensed in 'Monthly Journal of Medical Science' for 1854.)

The Normal Growth of Bone and its Rachitic Derangements. By R. Virchow.

7. *Beiträge zur Lehre von den Rückgratsverkrümmungen.* Von Dr. Eulenburg, praktischem Arzte und Operateur, Direktor des Institutes für Schwedische Heilgymnastik und Orthopädie zu Berlin. ('Journal für Kinderkrankheiten,' Band xxvi. § 47.)—*Erlangen,* 1856.

Contributions to the Doctrine of Spinal Curvatures. By Dr. Eulenburg, Director of the Institution for Iatrogymnastics and Orthopædics at Berlin.

HAD we sufficient space at our command, it would be no uninteresting nor uninstructive matter to trace the general history of "Rickets" from the times of Whistler and Glisson (*circa* 1645–50) to the present day; endeavouring to discover, too, on the one hand, why this affection has been regarded as peculiarly the "English disease;" and, on the other hand, assisted by Dr. Merei, to point out the very probable fact, "that 'rickets' is less frequent in English towns than in many of those of the Continent, and altogether of rare occurrence in Scotland and Ireland." (p. 185.)

But, *tempus brevis, opus multum, et paterfamilias urget*, and all we can do relative to the latter question is to refer the reader to Dr. Merei, thanking him ourselves for enabling us to reject the "soft impeachment" of continental writers. By the "medicine man" of "the brown forests of the Mississippi" alone we may perhaps be thought to have no cause for rejoicing, seeing that among some of the native tribes of North America a bowed form of the legs is esteemed handsome, the utmost care being taken in early infancy to mould the limbs by continued pressure into the admired shape.* A ring through the ear, or a bone through the nasal septum, a bowed leg, or a constricted thorax, *may* be, after all, but matters of taste; nevertheless, each race looks with favour alone on that particular deformity which it is the custom of its people to produce.

> "'Tis with our judgments as our watches, none
> Go just alike, yet each believes his own."†

The subject of *rachitism* has received its fair share of investigation at the hands of modern pathology, and of the *chief* results of which we shall now endeavour to give a general view.

Rachitis is an essentially developmental disorder, having its chief lesional manifestations in the bony tissues or system of support, and intimately connected with the process of the first dentition. It is allied to a general dyscrasic condition of the whole body, clearly evincing itself by a state of chloro-anæmia, and by a degenerate condition of the muscles, ligaments, and skin. In few words, however, the disease may be expressed as a softer state than normal of the bones in infancy and early childhood, which bones yield to pressure, or show divarications from their normal directions and positions in the frame. The child in whom rachitism in its full and perfect form is going to be manifested, becomes, within the first year of life, pale, has its flesh flabby, its digestive functions disturbed, and evinces more or less evidence of general *malaise*. The hair continues thin upon the scalp, the fontanelle wide open, and sweat breaks out upon the head. The little patient at length gets restless at night, bores its head into or rubs the back of it upon the pillow, and cries when the occiput is pressed, or the head lifted up and washed. If the head be now examined, the back of it may be found almost denuded perhaps of hair, the whole skull feels thinner than usual, and as if distended like a bladder, or it is flattened behind at the vertex, and protuberant in front. On careful manipulation with the fingers, the cranial bones may be found very soft in certain points, yielding elastically like cardboard beneath pressure, and

* Cummin, in Cyclopædia of Practical Medicine, vol. iii. p. 616.
† Pope's Essay on Criticism.

giving the feeling as if the skull might be bent inwards by the finger-points upon the brain. We have cut the calvarium (of a child six months old) with a small-pair of scissors as easily as cardboard, and folded the skull bones on themselves without breaking.* The disease progressing, the bones of the thorax begin to evince the rachitic state: the child cries when raised up under the arms, by the chest, or when its dress is tightly pulled around it, and the breathing on any excitement becomes embarrassed. Perhaps sickness and emaciation increase, and the child appears to suffer pain or inconvenience except when lying on its back quite still. The sternal extremities of the ribs are now found somewhat swollen or rounded out in a club-shaped manner, and the softened sternum projects more than it ought. In a short time the sternal ends of the *costæ* appear to fall in for some distance straight towards the spine, then to bend outwards towards the sides of the thorax, in a curve whose convexity is towards the vertebral column, so that beneath the axilla there is a large hollow or concavity instead of the normal and rounded form. Hence, what is termed *hühnerbrust*, or "pigeon breast," or "narrow chest," is formed, to which is sometimes added antero-posterior curvature of the spine. With still further pallor of the skin and flabbiness of the muscles, the carpal epiphyses of the radius and ulna become enlarged, which, with a greater relative increase of the metacarpal epiphyses than of the wrist-bones, gives the "knotted" or "double-jointed" appearance so frequently seen. Gradually, like conditions take place in the other joints, along with shortening of the long axes of the cylindrical bones, which appears to be the greater the smaller the transverse diameter of the shaft. The next step of the progress of the rachitic disorder is the curving and bending of the different bones from external pressure, the weight of the body, and the action of the muscles; and, lastly, the occurrence of "infractions," as some call them, or "bent-cracks" of the shafts. According to Vogel—

> "Of the humerus such 'infractions' are rare, but are more common of the femur, when a somewhat angular curvature, directed forwards and inwards, is to be observed. If such an 'infraction' be sawn through, on the convex surface, 'compact substance' alone, and on the concave face a thick layer of 'spongy substance,' are found; the medullary cells at the place of fracture are seen elevated by thick ossific growth, which, though afterwards becoming thinner, it is true, yet never permits of recommunication with the medullary canal."†

While the alterations relative to osteogenesis, which we have pointed out, are progressing, and giving rise to more or less change in direction, and to other deformities of the parts of the system of support, the muscles become flabbier and flabbier, the skin more pallid, often cold to the touch, or drier, and throwing off dirty furfuraceous scales. Relative to the structure of the muscles, however, if the observations of Vogel are to be relied on, it may be said that it undergoes no special morphological change, as in only one instance out of many examinations, was commencing fatty degeneration of the cardiac muscles seen, a condition which may be also met with in children who are clearly not suffering under rachitism. The bones of the pelvis may begin to yield and bend when the child endeavours (or is

* Lancet, vol. i. p 95. 1854.
† Journal für Kinderkrankheiten, Band xx. p. 184.

absurdly forced) to try and walk, either in consequence of a more or less partial curvature of the spine—the common cause, or from an inequality in length of the lower extremities—a less frequent circumstance. But the influence of rickets on the pelvis is as yet, it must be confessed, very unsatisfactorily made out. That the pelvis is often enough distorted in the female, is unfortunately too true; but that such distortion is of a rachitic nature is another matter. Trousseau remarks:

"Obstetricians of eminence, and in particular, Nägele, have at length asked themselves the question, Whether the pelvis of a rachitic female always exhibits deformities of such a character as prove with certainty their rachitic nature? and they have been the first (through the facts they have communicated) to oppose the rule that they had themselves established."*

"Rachitis can go through all its phases without prominently involving the bones of the pelvis, and generally has long ceased to rule where, in consequence of a prospective confinement, the pelvis of a woman perhaps undergoes examination; the rachitic child is not subjected to any of the influences which only mechanically operate on the pelvis of the parturient female; all the relations and dimensions are so little different, that a difficulty is rather found in pointing out the analogy than perchance any existing difference."†

Eulenberg‡ asserts, that next to deranged antagonism of the muscles, rachitism is the most frequent cause of *skoliosis*, 4·66 per cent. being due to it. The form of the curve is also said to be different in rachitic skoliosis, to what it is in the other variety, the *convexity* in the former being directed towards the left.

As may be readily imagined, various important functional and organic disturbances of the viscera encased by the rachitic bones, ensue; we can but barely allude to them. From the soft and yielding state of the skull, the brain is liable to pressure, and other irritation; and hence may supervene eklampsia, as we ourselves have witnessed. Elsässer and Lederer have described a *Tetanus apnoicus;* the latter also asserting, that out of 96 cases of "spasmus glottidis," 92 were associated with craniotabes; while Portal and Naumann allude to a form of *hydrocephalus acutus*, as arising from rickets. Of the *essential* connexion of these latter, much doubt may be had; but an overgrowth or hypertrophied state of the brain, with its results, are not uncommon. Dr. Merei states the enlarged brain is often strikingly soft and anæmic; but that whether so or not, the spinal cord appears to be abnormally "thin;" particularly in its lower portion, and most commonly softer than in the normal state—not seldom approaching a pultaceous consistence. In the form of the skull, an impress is bestowed, often for the whole of life, upon the countenance; the frontal protuberances and the parietal bones project as it were angularly out, and, as proved also by the measurements undertaken by Mr. Shaw, the bones of the face remain in backward development during youth, so that the adult has often a head of the form of a child. As might be expected, the interference to the functions and growth of the heart and lungs is often great in the extreme, the bones so compressing and displacing these organs, as to give rise to (in our opinion) the more dangerous complications which secondarily affect the rachitic child.§ Dr. Merei well observes:

* Journal für Kinderkrank., Band xvi. p. 256. † Op. cit., p. 267. ‡ Op cit., p. 88.

§ We may refer to a paper by Dupuytren, On Lateral Depression of the Walls of the Chest, as worthy of perusal—Sydenham Society's volume.

"When, in addition to the disorder in the joints and legs, the chest has assumed a notable degree of rachitic deformity—the well-known pigeon-breast, with lateral compression, even though without curvature of the spine,—then any slight bronchial affection is sufficient to form a serious complaint, on account of its being attended, more than in other subjects, with laborious respiration and impeded circulation, with a tendency to passive congestion, and carnification in the pulmonic parenchyma, venous hyperæmia of the brain and spinal cord, diminished innervation to the heart, cardio-spasmus—sudden death." (p. 146.)

According to some observers, the thymus gland is occasionally much increased in size; and Lagrange, Robert, and others, have specially dwelt upon the hypertrophied condition of the tonsils which is frequently present. The liver, as Glisson* pointed out, is very often enlarged in rachitis; but there is no doubt it often appears to be larger than it is, in consequence of the diaphragmatic stricture above it. This circular constriction, with the laterally appressed thoracic walls, and the often tympanitic intestines, may lead to an idea of such abdominal visceral enlargement, as by no means exists. Nevertheless, hypertrophy of the hepatic organ is not an uncommon accompaniment of rachitism; and although Glisson says: "Non male coloratum neque valde induratum aliove aliquo vitio notabili contaminatum, excipienda hic volumus," &c.,† modern pathologists have shown that "fatty induration" and "bacony liver" are occasionally to be met with. Hypertrophy of the spleen and "exsudation process" of the intestinal mucous membrane are also now and then observed.‡ Glisson, observes Dr. Merei,

"Mentions an anatomical alteration in some of the bloodvessels (as having been observed by one of his contemporaries, and which observation he believes to be correct)—namely, that the veins and arteries which run to the 'first affected' parts are uncommonly small, whilst the carotids and jugular veins are disproportionately enlarged. . . . . . So far as I know, this anatomical condition of the bloodvessels has not been noticed in our day; it has escaped at least, I must confess, entirely my attention in the dissecting-room." (p. 152.)

"The anatomical investigation of the dead bodies of rachitic children has not led as yet to the discovery of any such morbid alteration as might tend to a better understanding of the nature of this disease. . . . . The pathological alterations which are observed in those who die at an advanced stage of rachitism—namely, the anæmic aspect of the muscles and internal organs, a dark, over-carbonized appearance of the blood, and enlargement of the liver,—these alterations appear but as the immediate and necessary effects of the compressed state of the chest, and the impeded respiration and pulmonary circulation thereupon consequent,—these are the mere effects of mechanical compression, and devoid of other signification." (p. 156.)

"The learned professor of Cambridge [Glisson] must have seen a great number of very severe cases, and dissected many bodies of children dead of that disease; whilst at present, in spite of the infinitely more extensive medical observation, high stages of the disease and fatal cases directly from this cause seem to be a comparatively rare occurrence." (p. 153.)

Of the deposits of phosphate of lime occurring in rickets we shall speak further on.

In the preceding observations we have had in mind the manifestations of extreme general rachitism. Far less intense forms of the affection are

* De Rachitide, p. 49. † Op. cit. p. 12.

‡ Relative to these abdominal complications, see Bednar, Die Krankheiten der Neugebornen und Säuglinge, &c., Vierter Theil, § 46.

frequently seen, and occasionally the enlargement of the carpo-radial epiphyses and the curvature of the tibia are accompanied with so plump and fresh a condition of the child, that it is with great difficulty the parents can be induced to think there is anything amiss with it. Indeed, some authors have gone to the extent of dividing the disease into two distinct varieties—viz., one marked by a soft condition and curvature of the bones, and an attendant febrile dyscrasia; another, without such soft state, curvature and dyscrasia, and characterized by enlargement of the epiphyses, with a "fat and well nourished" condition of the child. The association of the different deformities with each other is also very variable, likewise the prominence of a particular deformity in different cases, as well as the amount of febrile disturbance and systemic depression. Some late observations made upon a highly acute form of rickets, and those of Elsässer* and Vogel† on *cranio-tabes*, "soft occiput," or rickets of the cranium, comprise some highly interesting points more particularly investigated by recent pathologists. To the former we shall make some reference afterwards; and in regard to the latter we would observe, that whilst rachitic *softening* of the cranium had almost entirely escaped notice until the observations of Elsässer and Widtman appeared, nay, was by some denied to occur, it is now shown to be very common, and to be, in general and extreme rickets, the first manifestation, so far as the bones themselves are concerned. We have seen some very severe cases, and recovery ensue; and never neglect to examine carefully and delicately the bones of the skull when an infant of from six months to a year old comes before us, and of whom suspicion of rachitism exists. This affection of the cranial bones must not be confounded with another peculiar state of the skull of young infants, first described, we believe, by Hauner, and denominated "undersliding occiput" (*untergeschobenes Hinterhaupt*). We were ourselves at first in danger of some confusion in the matter, but can now fully bear out the truth of the double series of observations. We may refer with satisfaction to the paper of Vogel in the twentieth volume of the 'Journal of Diseases of Children,' for much information relative to the two; and in the first volume of the 'Lancet' for 1854, some remarks pertinent to the question before us will be found at page 94.

We will now pass to the histological changes the bones themselves undergo. They may be divided into two great divisions—one relative to the stages of softening, or of *malacia;* the other to those of hardening, or of *eburnation.* The propriety of the term *softening*, as applied to the bones in rickets, is doubted by some; by Virchow, for example, as we shall afterwards see, and by M. le Professeur Gerdy. But whether the bone ever undergoes a process of actual softening after it has been hard, or whether always it has never hardened as it should do, but remains soft, may perhaps admit of discussion. The newly-developed bone-tissue illustrates the latter, that formed before rachitis commenced, the former process. M. Gerdy maintains that when true softening occurs, a *complication* is added to the rickets. Under any circumstances, the bones may be both conveniently and truly said to be in a *soft condition,* though Virchow's views we hold to be correct so far as the explanation of how such

* Der Weiche Hinterkopf, &c. Stuttgart und Tübingen, 1843.
† Journal für Kinderkrankheiten, Band xx. 1853.

a condition *mainly* arises is concerned. If a cylindrical bone of a child who has died with *cranio-tabes*, or well-marked rickets, be examined, the periosteum will be found twice or thrice as thick as normal; in some places highly injected, in others discoloured or marked with milky spots. On its removal the bone looks red, or may even be of a violet colour, is unequally rough, and the *foramina*, as well as the vessels entering them, appear somewhat enlarged in dimension. On section of the bone a quantity of rather tenacious sanguinolent serum is seen to be poured out between the bone and periosteum (where the latter is present), between the bone and cartilage, and even into the osseous tissue itself. If a bone of a still more advanced stage of rachitism be examined on its transverse section, the "compact substance" is found lying towards the interior, whilst in the normal condition it is situated externally. According to Guerin, the sero-sanguinolent liquid before alluded to is at first as thin as water, and easily to be washed away from the denuded periosteal surface of the shaft; but Vogel doubts this, having always found this fluid to be—

"A tenacious red mass, deposited in greatest quantity at the periphery of the bone where it is bounded by cartilage and periosteum. At a later period of the first stage [of softening] this mass coagulates into a firm jelly, in which a new vascular *rete* is organized; so that at length the entire superficies of the bone appears covered with a jelly, containing a thick vascular network."*

At a still later period the epiphyses are found swollen, and the line of demarcation between bone and cartilage is jagged, or irregularly waved, instead of being straight and regular. The minuter changes which occur in the tissues of the system of support have been very closely inquired into of late years by Virchow, Kölliker, Meyer, Vogel, and Stiebel. Virchow's views are very generally accepted, and these, as still further illustrated by Stiebel, will guide us mainly here in our further remarks. To fully appreciate the details of the histology of rachitic ossific tissue, it is very essential the normal structure and growth of bone be kept in view, and the process of osteogenesis, as developed by Sharpey and Virchow in particular, should be carefully borne in mind. It must be remembered that a bone grows both in length and in circumference; in circumference by virtue of a sub-periosteal deposit of what Virchow has called "pumice-stone-like osseous matter," and in length by means of renewed strata of cartilage-cells deposited between the epiphyses and cartilage, and in which cells osteogenesis commences nearest the bone. The "pumice-stone matter" is neither compact nor spongy substance, but something between them, containing large spaces becoming filled with concentric layers (when at an after period compact matter, as it is termed, is formed), and finally going to form the small Haversian canals. During the growth of a bone the central medullary canal becomes increased in size, so that the shaft of a bone in the child might be lodged in the medullary cavity of a like bone of the adult. Now as this increment can only ensue through absorption of the innermost layers, the earlier formed compact substance must gradually approach the central cavity, and become absorbed. We have therefore osteo-formation peripherally, and osteo-absorption centrically, the entire process of osteogenesis and growth being resolved into—

* Journal für Kinderkrankheiten, Band xx. p. 166.

1. Deposit of sub-periosteal, or soft "pumice-stone matter."

2. Transformation of this matter into "compact substance," by the formation of lamellæ in the larger spaces.

3. Resorption of this compact substance, and its metamorphosis into spongy matter in connexion with the increasing diameter of the central medullary canal.

Vogel remarks,

"According to Virchow's doctrines, the whole pathologico-anatomic changes met with in rachitic bones rests on the defective transformation of the 'pumice-stone' mass into compact substance; and this view is so simple, and apparently so true, that we must adopt it unlimitedly, preferring it to all other theories."*

So far as the development of a bone actually proceeds, it ensues in the same manner in rachitism as in health, according to Virchow and Stiebel. There are the same changes, the increase of the cartilage by endogenous cell-formation, the appearance of medullary cavities, the formation of osseous corpuscles, but the ossific process is arrested, for true ossification does not ensue. Relative to the longitudinal growth (or that by epiphysis) of a bone, the histologic differences observed as dependent on this arrest of ossification, are,

"1. Arrestation of the line of ossification, with a relative increase of the preparatory growth-line of the cartilage. In the hyaline layer of the large-celled cartilaginous matter a delicate granular-striped network of intercellular substance is formed, in which lie thick-walled cartilage fossæ, containing the pale cartilage-cells provided with nuclei and nucleoli. The great breadth of the preparatory cartilage-stratum is explained by the dilatoriness of the process of ossification.

"2. Intrusion of the medullary spaces within the line of ossification, or even beyond it, with persistency of the growth of the cartilage.

"3. The formation of fibrous medullary spaces; and the osteoid transformation of their surrounding and distant parts, without the deposition of any calcareous matter. The walls of the cartilage-cells become thickened, their cavities diminish in size, and the bone-cells, with their canaliculi, are formed, but without the deposition of a salt of lime between them."†

According to the investigations of Meyer, yellowish opaque spots arise in the proliferous matrix of the cartilage, in which fibrous formation and softening proceed until vacuities are formed, only to be distinguished from the normal medullary spaces by not containing any lime in their parietes. This view would somewhat modify the above proposition (No. 2).

Connected with the circumferential growth, the following points are the more essential to be dwelt upon:

"1. Increased growth of the sub-periosteal formation, with continuous metamorphosis of the substance into areolæ and cancelli.

"2. Defective ossification of the cancelli, with persistence of the deeper layer of compact margins. The serratures and arches of the areolar bone-stratum are quite devoid of calcareous deposit, looking like shining courses of thick matrix with jagged osteal cells; the deposit of lime is first observed in the deeper layers, and proceeding from within outwards. The rachitic bone has, like the normal one, its thickest layer beneath the surface, but in the latter the layer of 'deposit' is thicker.

"3. Partial cartilage-formation within the areolæ. In the layer of deposit a

* Journal für Kinderkrankheiten, Band xx. p. 164. † Stiebel, ibid., b. xxii. p. 365.

cartilaginous mass sometimes appears, identical in structure with the growing cartilaginous substance of the epiphyses."*

According to Trousseau, if an unbroken rachitic bone is compared with the "healing process" of the fracture of a sound one, a striking resemblance is observed between the first stage of such process and the rachitic condition. The process of ossification is almost identical in both, with the exception that in the latter it proceeds more slowly than in *callus*.

The process of ossification being, however, established and progressing, the bones at length become hard, the stage of *sclerosis* is attained, and in many cases that of *eburnation*, or the assumption by the bone of a very hard and white ivory-like appearance. Microscopically examined, the tissue is found possessing all the characters of "compact substance," but containing comparatively few medullary canaliculi. Whilst before an arrestation of ossific action was seen, now perhaps excessive, osteogenesis occurs. Not only are the sound bones more dense, but sometimes hyperostosis near the epiphyses and sutures is observed. Copland has seen this occur at the terminations of the ribs and commencement of the cartilages, so that the whole was more or less soldered together with the sternum;† and Stanley has observed the bone at the point of curvature converted into a solid substance, the medullary cavity being wholly obliterated.‡ The cause of the latter occurrence has been stated§ to be the bending of the bone compressing into the interior of the curve a larger quantity of cartilaginous substance, which is afterwards filled with earthy deposit, while it stretches and attenuates that on the exterior of the arch, and so diminishes there the matrix for the reception of the earthy materials. In reference to this part of our subject, Mr. Bishop remarks:

"Wilson observed that when the bones begin to recover from the disease, the deposition of osseous matter is most actively carried on in those portions of bones where it is most wanted—that is, 'on the inner or concave surface of the curve;' and Mr. Stanley not only confirms Wilson's remarks, but further states that the thickness of the bone at the part most curved bears an exact ratio to the degree of curvature that the bone has undergone. The deposition of ossific matter is not always, however, adjusted to the mechanical conditions of the parts of the body as means to ends; on the contrary, the bones of the skull are often thickened far beyond their normal state and what is necessary for the protection and support of the brain under ordinary circumstances. It is stated that Sir Charles Bell had in his museum rickety subjects, in which the parietal bones were seven-eighths of an inch thick at their central parts."||

As regards the history of the lesional changes in the bones in rickets—i.e., the deficient transformation of tissue into "compact substance," and the insufficient deposit of the phosphate of lime in its walls—the chemical inquiries of Fourcroy, Davy, and Bostock may be said to have first illustrated the latter point. Dr. John Davy found 100 parts of the dry tibia of a healthy subject of fifteen years to yield 46·4 of animal matter, and 53·6 of earthy; while the same quantity of the dry tibia of a rickety child contained 74 parts of animal, and 26 of earthy substance. In later years, Marchand, Lehmann, Schmidt, J. Müller, Ephraim, Von Bibra,

* Op. cit., p. 364. † Dictionary of Practical Medicine: art., Rickets.
‡ Medico-Chirurgical Transactions, vol. vii. § Cummin, op. cit.
|| Lancet, April, 1848.

Stanski, and Dreux, have subjected rachitic bones to analysis, and found the bone-earth often but one-fifth instead of two-thirds of the dry bone. The last analysis we are aware of is one by Professor Böttcher, of the bones of a patient who died of "acute rickets," in the stage of softening, in the Frankfort Hospital, under the care of Dr. Stiebel. The bones were those of a male child, thirteen months old. The femur yielded 20·89, the tibia 24·70, the radius 20·00, the rib 26·65 per cent. of inorganic matter, instead of 60 or even more. Schlossberger, who has lately closely investigated the changes going on in the bones in "craniotabes," states that, as a general result of his whole examinations, the inorganic material was found in the thinned parts of the bone to have sunk to 53 or 51 per cent., and in the thickened spongy parts to 28 per cent., or in slight cases to 40 or 43 per cent. In bones of a normal state, completely dried at a temperature of 120°, the proportion was found to range from 61 to 68 per cent. As the affection becomes cured, the proportion augments, but never to the extent, says Schlossberger, of exceeding the normal one. The proportion of carbonate of lime to that of earthy phosphates is very fluctuating, and that of the fatty matters is not materially changed.

As to what alteration ensues in the cartilaginous basis of the bone in rickets, some difference of opinion has existed. Some deny any change as essential to the disease, or even as commonly present; while at a late period, as affirmed by Simon, "the animal matter is so changed that its extract yields on boiling neither chondrin nor gelatine" [glutin]. It has been maintained by many that the non-existence of any change in the cartilaginous basis is the great mark of distinction between rachitis and osteomalacia, and of which latter such a change is so essential a characteristic.

We may now ask the question as to what is the cause of this deficient solidification of the bones, or the want of the phosphate of lime in them, in rachitism?

"M. de Fouchy by chance saw an ivory spoon which had been left, forgotten for a long time, in some milk, which, when found had become as supple as leather. This suggested to Hanauld that it was the acidity of the milk which had effected its mollification; and to ascertain the truth of his surmise he immersed some bones in vinegar, which softened them; he then counteracted his experiment by soaking the same bones in water for some time, and drying them, when they resumed their natural induration; and to conclude, he again rendered them soft by a second immersion in vinegar. These facts were deemed sufficient to establish the presence of an acid in the blood of rickety patients, which, according to these theorists, dissolved the medium which held the earthy component of the bones adherent. We then find this opinion adopted by Gagliardi, Herissant, Ruysch, Morand, and many others; but on the strength of an analogical inference, not logical proof."*

Fourcroy long ago asserted that the phosphate of lime taken in with the nutriment was dissolved by a free acid already existing in the system, and carried off by the urine; so that it could never get into the blood as such, and therefore, as such, could not be deposited in the bones. The fact of the urine often containing and depositing phosphate of lime in

* A Treatise on Rickets, with a new Theory of Ossification, by G. Hume Weatherhead, M.D., &c. Second edition, p. 87. London, 1835.

considerable quantity, and the assertions of some pathologists that it was also stored up in the mesenteric glands (a statement now generally regarded as far from the truth), helped to support this theory of an acid dissolving (by excess of itself), either in the stomach or the blood, the salt of lime, and causing it to be deposited elsewhere; whilst others even supposed the excess of acid caused the removal of it from the bones after it had been deposited.

Some modification of this doctrine has been the one generally received, and Dr. Stiebel himself supports it. If the question be asked, What free acid is this that does so much mischief? Marchand and many followers answer, It is *lactic*, formed by the powers of the gastric mucous membrane from the milk sugar, grape sugar, gum, and starch, taken in. Beneke, Ure, and Schmidt, on the other hand, regard *oxalic acid* as the one; Weatherhead thinks it is *phosphoric;* and some have thought that after all it is *hydrochloric*. In all these acids, it is true, the phosphate of lime is easily soluble. When the cause of the *persistent* generation of this free acid has been asked for, the answer has been, "an abnormal charge of the portion of the nervous system regulating the functions of the stomach;" or "an increased excitability of the gastric mucous membrane;" or "improper nourishment, or over-feeding with farinaceous or starchy food;" or "too animal diet," &c. According to Dr. Merei:

> "In the action of the saliva itself, which is so copiously secreted in children, we find an explanation of this fact, insomuch as saliva, in its normal condition (by the action of its principle—*ptyaline*), converts starch into sugar and sugar into lactic acid; to the superabundance of which Marchand ascribes the prevented solidification of the phosphate of lime into bony structure." (p. 204.)

That phosphate of lime is not deposited in sufficient quantity in the bones—that an excess of earthy phosphates often exists in the urine—and that a superabundance of free acid is constantly generated in the *primæ viæ*—we hold to be facts fully proved as existing in rachitic children. But that there is more in the matter than the mere excessive generation of acid in the stomach, or its presence in the blood, is clear to our mind. How is it, as Behrend asks, that all children who suffer from acidity—and their name is legion—do not present some signs of rachitism? Admitting, too, that the phosphate of lime intended for the bones passes off by the kidneys, yet the latter must obtain it from the blood; why, then, does the blood carry it on, and not deposit it in the ossific tissues? The only attempt to answer the latter is made in Weatherhead's exposition,* which assumes the free acid to be phosphoric. Further, how are we to account for the wandering pains in the bones in the first and second stages of the acuter varieties; the peculiar chloro-anæmic appearance and cachectic condition of the child; the occasional early outbreak of the malady under no impropriety of diet; and finally, the sometimes almost sudden arrestation of the disorder, when it has run through certain determined stages, without any therapeutic or even hygienic interference of importance?†

The unsatisfactory chemical evidence of the existence of lactic and

* Op. cit., p. 86.

† Behrend, in Journal für Kinderkrankheiten, Band xxii. § 366; Copland's Dictionary of Practical Medicine: art., Rickets, 30, C.

other free acids in the blood, and some late minute anatomical investigations of rachitic bone, have been the means of inducing one or two pathologists to advance a very different ætiology to that generally received. Böcker finds the cause of rickets to be mainly in a too rapid action of the "blood-moulting" process. According to Trousseau and Lasegue the primary fault has its origin in the bones themselves. There is set up in them a special form of inflammatory action, having as its result an absorption of the already deposited salt of lime on the one hand, and serving as an hindrance, on the other, to its further deposition. The fundamental lesion, or primitive morbid action, at the base of rickets, is affirmed by Meyer to be a "hyperæmia of the periosteum or periostitis;" and in proof of which he states he has been able to demonstrate, not only different exudation products, but, in two cases lately observed, even purulent deposits beneath the periosteum. One of the latter cases

"Had a collateral interest, inasmuch as it was one of a new-born child from whom the remains of the umbilical cord were not yet detached; it was, therefore, an undoubted case of *intra-uterine* rachitis. The larger bones of the extremities (and even most of the other bones, especially the scapulæ and iliac bones) exhibited in this child the following remarkable conditions:—Upon a firm and compact *substantia dura* was superimposed a very firm and thick *osteophyt*, cellular, and rough externally. The macerated shafts represented in miniature the thickness, weight, and unevenness, or almost the appearance, which one is accustomed to see in bones with *complete necrosis*, only, of course, neither *cloacæ* nor *sequestrum* being present. Where the diaphysis and epiphyses met, the periosteum was distended, the epiphyses exhibiting quite an unusual mobility. On opening the periosteum at this point, a cavity was seen filled with a thick, fluid, chocolate-like matter. The microscope demonstrated the existence of crumbled detritus, blood, and pus-cells. In this cavity lay the end of the shaft, completely disengaged, and surrounded by the fluid; and it was thus clear that a purulent deposit from periostitis had loosened the entire cartilage, which, at the margin of ossification, had undergone growth by primary cells, but in consequence of the disease had remained *unossified*. At the end of the shafts, bathed in the pus, 'corrosion' was observed in some places; but, in general, the shaft ends were very little affected as yet, and even possessed the thin osseous lamella separating the plane of ossification from the non-ossified parts."*

Whilst we admit the want of *demonstrative* evidence in proof of the existence of lactic acid in the blood, or excess of other acids in the stomach, in rickets, we are far from willing to admit a local inflammatory action of the periosteum or bones in this disorder as its fundamental essence. We acknowledge the force of much of MM. Trousseau and Lasegue's reasoning; nor do we dispute the statement of Meyer, that he has seen exudation products, and even purulent matter, in a bone belonging to a rickety skeleton: but even admitting that *inflammation* did play a part in the train of phenomena, we should require far more stress to be laid upon an earlier *general dyscrasia*, than the pathologists of the school we have alluded to appear to concede. We have general affections of the bones and periosteum in syphilis and arthritis, but then there is something far more fundamental at which we can arrive than a merely local lesion of the tissues of the system of support. So it is in rachitis. We do not deny that some of these pathologists refer to "too-early weaning," "improper nourishment," "damp and ill-ventilated habitations," &c., as liable to predispose

* Henle und Pfeuffer, Band vi. § 151.

to rickets; but we do find more stress placed upon the "local hyperæmia," than on the diathetic causes we shall presently refer to. We are thus brought to inquire into the nature and relations of the constitutional state giving rise to, or accompanied by, rachitism, and into its identity or connexion with one or two important diathetic conditions.

Until about twenty years back, the opinion was very generally adopted, that rickets was *scrofulous disease of the bones.* The fundamental dyscrasia preceding and accompanying lymphadenitis, disease of articular cartilage, certain common affections of the tegumentary surface and its appendages, and all forms of tuberculosis, was regarded as that at the bottom of rickets. Some, however, doubted this; and at length, in 1834, M. Rufz asserted that he had examined the bodies of twenty rachitic children, and had found but six times pulmonary tuberculosis;* and M. Trousseau subsequently added, that not five per cent. of rachitic children were scrofulous. It was afterwards maintained,† that not only is rachitis not scrofula, but that the former appears actually to exclude, in some manner, tuberculosis; and lately, M. Trousseau has taken up the same argument:

"Rachitis and tuberculosis appear to exclude each other; for whilst in children labouring under chronic diseases generally, tuberculosis exists in the majority, scarcely one would be found with pulmonary tubercle in twenty rickety-cases. Between scrofulosis and rachitis there exist such striking differences, that while the former is almost always incurable, the latter is pretty generally self-curative, and gives rise to death only by intercurrent affections. Scrofulosis, on the other hand, leads to the deposit of tubercle. Sometimes rachitis is confounded with mesenteric scrofula."‡

Dr. Merei states that in Manchester, where rachitism is frequent, tuberculosis is seldom observed in children; and which fact agrees with his experience at Pesth, where, in upwards of five hundred post-mortem examinations, not one instance occurred of tubercular consumption in a child having a high degree of rachitic spine and chest. The author concludes

"That certain forms and certain degrees of scrofulosis and tuberculosis *may* co-exist with rickets; but that a high degree of rachitic compression of the chest necessarily connected with over-carbonization of the blood, is a condition adverse to the development of pulmonary tubercles. The assertions, therefore, of Rokitansky on this subject seem to be correct. . . . . Finally, my conclusion, based upon careful observation, is, that rachitism is a disease which *may* be associated with, but is distinct in its nature from, scrofulosis and tuberculosis." (p. 197.)

For further information upon this immediate point, the reader is referred to some observations by the writer of this article in the 'Lancet' for August 26th, 1854. We have formed a strong opinion upon the question at issue, obtained after some years' experience amongst the poorer children of a not unimportant part of this metropolis. We would admit, of course, that, so far as the local lesion is concerned, rachitism is not tuberculosis, nor is tuberculosis rachitism; and we would maintain that tuberculosis is not properly a convertible term with scrofula, nor is rachitism. Scrofula is a "general," of which tuberculosis (in the opinion, at least, of very many pathologists) is a "particular,"

* Bibliothèque du Medécin-Praticien, tome vi.: art., Rachitisme
† See Bouchut, Maladies des Nouveau-Nés, p. 804.
‡ Journal für Kinderkrankheiten, Band xvii. p. 246.

and (as we think) so is rachitis. The two forms of disease are local and particular manifestations of a general dyscrasy, which we believe is the same in both—viz., the scrofulous. That tuberculosis is not a *frequent* manifestation, going hand-in-hand with rickets, may be granted, just as well as that marked chronic lymphadenitis, ulceration of articular cartilage, &c., are daily seen to be unaccompanied by tuberculosis; and which fact, we are aware, has, along with other reasons we have not space to touch upon, led some pathologists (like Lebert and Legrand) to maintain that the latter has no essential relations to the scrofulous cachexia.* On the other hand, Laycock, Balman, and Glover have shown† that so close is the connexion between them, that an external manifestation of the scrofulous diathesis is a defence against the other, or acting as a sort of safety-valve to the lungs. That tuberculosis and rachitism do not absolutely exclude each other, is known to us by necroscopic inquiry; indeed, we have met with one of the rarest forms of the deposit—viz., intraserous grey granulation of the pleura—in a child one year and a half old, with rickets;‡ whilst M. Hervieux having met with the coincidence so often as one in three, is actually led to regard rickets as an important symptom of tuberculosis before three years of age.§ To our minds, that which would reject rachitis as an evidence of the scrofulous cachexia would go far to the rejection of tuberculosis as such, too; and that this has been done by some we have already hinted. The like reasoning also which leads us to acknowledge tuberculosis to be connected with the dyscrasia in question, would incline us to view as such rickets too. Its occasional union with tuberculosis, its not unfrequent complication with the tegumentary, &c., forms of scrofulous inflammation; the occurrence and hereditary transmission of all three manifestations in intense form in the children of the same family, not one of whom escape one or other variety, and one if not both of whose parents are evidently strumous, lead us to this opinion. The manifestations of the scrofulous cachexia, as thus indicated, no doubt appear in themselves to be often of a very opposite character; but let us take a cognate malady—syphilis,—and ask if we were less acquainted than we are with its natural history, of its transit from primary sores to eruptions, to affections of the cutaneous surface and mucous membranes, to glandular enlargements, and through these to wandering nocturnal pains of the bones, to periostitis, to caries, to necrosis, &c., &c.; its transmission to the fœtus in utero, and final culmination in the production of abortion;—should we find it much easier to believe these different manifestations to be truly the offspring of a common origin, than those of the scrofulous cachexia above alluded to? The difficulty here suggested partly existed, indeed, during the sixteenth and seventeenth centuries, as Behrend of Berlin has pointed out.

Boerhave and several writers since his time have actually regarded rickets as belonging to the syphilitic rather than to the scrofulous cachexia; while others have referred the latter itself to syphilis. Vogel says the father of a rickety child has frequently acknowledged to him his syphilitically-tainted system; and thinks the existence of the latter may fre-

* See a review of various writers on the Pathology of Tubercle and Scrofula, in the fourth vol. of the new series of this journal (Oct. 1849); also Rilliet et Barthez, tome iii. p. 314 et seq.
† British and Foreign Medico-Chirurgical Review, vol. x. p. 187.
‡ Lancet, ut antea. § Rilliet et Barthez, tome iii. p. 766.

quently explain the occurrence of rachitism in the children of the better classes. On this point Trousseau observes:

"According to the views promulgated by Ricord upon congenital syphilis, and on the transmission of secondary and tertiary accidents, which may come, after several generations, to represent scrofulosis, rickets may perhaps be descended from the same. In the present day the latter is far more frequently met with among the poor than the rich. With the former, in consequence of neglected treatment, and the operation of external deleterious circumstances, syphilis becomes more obstinate and rooted in its transmission to the following generations; so that rachitis, as Boerhaave, Van Swieten, and Glisson have supposed, may be a degenerate form of syphilis."*

While from our own experience we should be disposed to lay more stress upon the transmissibility of a defective constitution from parent to offspring as the first link in the chain of causation of the general dyscrasia, whatever name it may receive, others, admitting the occasional transmission, lay more stress upon the operation of external causes after birth, regarding the diathetic state rather as *in toto* acquired than as necessarily associated with an hereditary predisposition. We by no means stand alone, however, in this belief of the transmissibility of the dyscrasia of rickets from the parent to offspring. Hennig† affirms true rickets, or osteopsathyrosis, to be in the highest degree hereditary; while, according to Schönlein, too early marriages, and to Küttner, intermarriages, mainly conduce to its transmission.

In reviewing the numerous extrinsic circumstances brought forward as the cause of the cachexia of rickets, it is interesting to observe how they are all those which are more or less intimately associated with the manifestations of the scrofulous dyscrasia—viz., deficient or improper diet, bad air, moisture, cold, want of exercise, and deficient exposure to solar light, &c. Of the relative import of these circumstances in the causation, much difference of opinion exists. The experiments of Guerin on the lower animals go to prove the influence of improper food; while Dr. Cummin observes (op. cit.) that whole broods of young geese and ducks, young pointers and greyhound puppies, and young pigs, have the rickets, or "krinckets," when they have been continually exposed to cold and wet, or have been kept in damp kennels and sties. According to Trousseau, damp and ill-ventilated habitations "give rise to scrofula rather than to rickets;" while in M. Coste's experiments they tended specially to tuberculosis. M. Trousseau states that of a hundred rickety children ninety-eight were either never suckled at all, or were weaned very early; and Mr. Lonsdale is of opinion that rickets—

"Is produced by deficient nourishment during the period of infancy, when the child does not get sufficient from the mother, either in the quality or the quantity of the milk secreted. I have paid particular attention to this point, and have found, I think invariably, that in all rickety children the parents have had little or no milk for their supply, and that they have been obliged to feed them either partially or wholly. The mothers will tell you that the children never grew properly from the first. I do not mean to say that children brought up by hand necessarily become rickety, for a child may thrive well if *properly* fed, and may be strong enough to thrive even if *improperly* fed. This is found by every-day experience. But I believe that a child will not become rickety if the mother be a healthy woman, and has plenty of milk secreted to supply the kind of food nature intended

* Journal für Kinderkrankheiten, Band xvii. p. 247.

† Lehrbuch der Krankheiten der Kinder, § 120.

during the first twelve months of infancy. A healthy child, however, though born of healthy parents, may become rickety if taken from its mother and brought up by hand with *improper* food; while, on the other hand, a child becoming rickety may be restored to health by being put to a strong healthy wet-nurse. I have seen many cases of this kind where, I am sure, this has saved the children from future deformity."*

Böcker, in 1849, published a series of cases, tending to show that it was probably a deficiency of the phosphate of lime in the maternal milk which gave rise to rachitism in cases where the child was suckled. Drs. Vogel and Merei attach much importance to impure air. The former writes:

"Of extrinsic causes, one can alone be affirmed with surety—viz., the want of fresh air, which is universally shown by all observers to be the most frequent in causation of rickets, and evinces its importance by the fact of the infrequency of the disease in southern climates, and during the warmer quarters of our year."†

Dr. Merei believes (p. 186) improper food *alone*, under the influence of a pure atmosphere, to be insufficient to produce rachitism; while the influence of unwholesome air is sufficient to give rise to it, in spite of the most wholesome kind of diet. Hauner regards "great poverty, insufficient clothing, and want of exercise," as more powerful than improper food, though "a highly-carbonized air, as shown by Beddoes and Withering, is equally detrimental."

To our mind, the above are all only so many extrinsic exciting causes (each of considerable, yet, according to circumstances, frequently variable, force) of the scrofulous dyscrasia, which may evince itself by different open manifestations, according to circumstances as yet imperfectly known to us. In one child, chronic abscesses or ulceration, in another, tubercular deposit, and in another, rickets, may make their appearance; the more intense the particular form of outbreak in each case, the less chance perhaps that one class of manifestation shall be complicated with another. In connexion, finally, with this portion of our subject, we have still left for us to inquire into the relationship between rachitis and the *osteomalacia* of adults, a relationship firmly believed in by a party, of which we might take M. Bouchut as the exponent, when he says: "I agree with Boyer and M. Beylard: rachitism and osteomalacia are one and the same disease of the bones, modified by the age of the subjects; for me, rachitism is the osteomalacia of infancy;" and Vogel as that of another, which declares—

"Rachitis and osteomalacia are evidently two different processes—the one quickly progresses in developing bone; the other attacks bones of completed development: the one passes through certain stages to a given point only, and reverts, even if slowly, to a state of health; the other induces various changes, particularly fatty degeneration of the bones (of which nothing is observed in rickets), and progresses irresistibly to its ever-fatal termination."‡

We are compelled, however, here to draw our observations to a close, trusting on some future occasion to be able to revert to this particular portion of our subject, as also to that of *acute* rickets. In concluding, we would strongly recommend the perusal of a paper by M. Trousseau, in reference to these matters, and which will be found in the seventeenth volume of the German 'Journal for the Diseases of Children.'

* Lancet, Sept. 1855. † Op. cit. p. 168. ‡ Op. cit. Band xx. p. 164.

W. Hughes Willshire

Review V.

1. *On the Capacity of the Lungs, and on the Respiratory Functions.* By John Hutchinson, Esq. ('Medico-Chirurgical Transactions,' Vol. xxix. 1846.)
2. *On the Mechanism of Respiration.* By Francis Sibson. ('Philosophical Transactions.' 1846.)
3. *Ueber die Menge der Ausgeathmeten Luft bei verschiedenen Menschen.* Von Gust. Simon.—*Giessen*, 1848.

On the Quantity of Air Expired by different Persons. By Gustavus Simon.

4. *Fabius, de Spirometro ejusque usu dissertatio.—Amstelod.* 1853.

Fabius, Dissertation on the use of the Spirometer.

5. *Krankheiten der Respirations-Organe.* Von Dr. M. A. Wintrich. ('Handbuch der Speciellen Pathologie und Therapie,' Band v. Abthl. 1.)—*Erlangen*, 1854.

Diseases of the Organs of Respiration. By Dr. M. A. Wintrich.

6. *Ueber die Athmungsgrösse des Menschen.* Ein Beitrag zur Physiologie und zur Diagnostik der Krankheiten der Athmungswerkzeuge. Von Dr. Friedrich Arnold.—*Heidelberg*, 1855.

On the Capacity for Respiration in Man. A Contribution to the Physiology and Diagnosis of the Organs of Respiration. By Dr. Frederick Arnold.

That respiration is a function indispensably necessary, not only to the preservation of health, but even to the very continuance of life, is remarkably exemplified in the provision made for it in every living being. Throughout almost the entire animal kingdom we find distinct organs for the aëration of the blood; and these organs cannot be either temporarily or permanently interrupted in the performance of their peculiar office, without producing a corresponding transient or permanent disturbance in the vital functions. Although the structure and situation of the parts ministering to the respiratory process differ widely in the various species of animals, yet the duty devolving upon them all—namely, the absorbing of oxygen and exhaling of carbonic acid—remains invariably the same, whether it be performed by the agency of lungs, as in Mammals; by gills, as in Fishes; by tubes, as in Insects; or by simple sacs, as in some Gasteropods. The lungs, gills, tubes, and sacs form, however, but a small portion of the apparatus necessary for the interchange of the gases in the animal economy; and if we consider respiration in its more extended acceptation, solely as the process by which oxygen is absorbed and carbonic acid exhaled, we are forced to acknowledge that every individual organ and tissue of the body respires; and consequently, that the so-called respiratory organs are far from being the only scene of the breathing process. Few of our readers will, we think, be inclined to doubt that a knowledge of the changes which the air undergoes during respiration is

of the utmost importance to the preservation of health; and many will unite with us in support of the opinion, "that a clear comprehension and investigation of the action of oxygen in the human body, is of no less practical importance than the great and fundamental principle of the circulation of the blood."*

Every new discovery, therefore, which tends to elucidate any of the more obscure points connected with the study of this interesting and difficult subject, will be welcomed alike by the practical and by the scientific members of the profession.

Were we to consider the mechanism of respiration simply as the means by which the gases are carried to, and expelled from, the pulmonary air-vesicles, we might, after the perusal of Dr. Sibson's beautiful and learned researches upon the movements of the chest, be inclined to believe that a perfect knowledge of the respiratory mechanism had been attained. But inasmuch as this function is to be regarded simply as the absorption of oxygen and exhalation of carbonic acid, and as we acknowledge that every organ and tissue respires, we are forced to admit the necessity, not only of a clear appreciation of the method in which the air is conveyed into the vesicles of the lungs, but also of an acquaintance with the laws by which it traverses their walls, enters the general circulation, is transported to the capillaries, and finally received into the tissues, to become incorporated with them in the act of assimilation; all these points must be thoroughly understood before the mechanism of respiration can be definitively discussed.

The valuable researches of Professor Graham on the diffusion, the scarcely less brilliant ones of Henry and Dalton on the laws of the absorption, of gases, and the experiments of Daniel, Draper, and others, on endosmose and exosmose, have gone far towards elucidating the subject under consideration. The question, however, still presents sundry important points on which physiologists differ. One sect, with Valentin for their leader, declares that in respiration the gases traverse the walls of the pulmonary air-vesicles, and enter the blood in obedience to the same law by which they would pass through a dead animal membrane or a porous inorganic substance—viz., that of inverse proportion to the square roots of their densities. But this view, applied to our subject, requires much further consideration; for in respiration the gases are placed in very different relations to each other from those they occupy when both exist in a gaseous state, with only a dead animal membrane or inorganic porous material intervening between them. In the lungs the gases exist, *in a gaseous form*, on one side only of the separating membrane; on the other side they are mixed, and even to a certain extent chemically combined, with a liquid whose presence may entirely nullify the law of diffusion. This is, in reality, proved to be the case. Graham's law of the diffusion of gases in inverse proportion to the square roots of their densities, only holds good providing the intervening membrane be perfectly dry. The smallest degree of moisture immediately annuls the law of diffusion, substituting for it the law of absorption. This we ourselves have had occasion to see beautifully exemplified in an experiment, where

* Dr. Bence Jones's Animal Chemistry, p. 7.

the law of diffusion between carbonic acid gas, on the one side, and atmospheric air on the other, was intended to be demonstrated; but where, in consequence of some moisture accidentally adhering to the membrane separating the two gases, the law of absorption had come into play Instead, therefore, of seeing, as was expected, the air, the lighter of the two gases, going over in greater proportion to the carbonic acid, the heavier gas, a diametrically opposite effect was observed; the carbonic acid went over in greater proportion to the air, the absorbing power of water being stronger for carbonic acid than for atmospheric air.

If, then, a single drop of water is sufficient to render null and void the laws of diffusion, how can we entertain the idea that the blood, whose liquid particles are nothing more or less than water, and that the moisture in the walls of the pulmonary air-vesicles can produce any other effect? Not, certainly, because the membrane in the lungs is living, while that in the above-mentioned experiment was dead. For it has yet to be shown that a living membrane is endowed in this respect with properties not possessed by a dead one, whose tissues are yet unchanged by the process of decay. Science has till now been unable to detect any such difference.

The other sect, at whose head we think Vierordt may be justly placed, bases its theories upon the laws of absorption discovered by Henry and Dalton. Of these two observers, the first has pointed out how the volume of a gas, absorbed by a liquid, depends on, or rather bears a direct proportion to the atmospheric pressure. If, for example, a given quantity of water, under a pressure of one atmosphere, has the power of absorbing 11 volumes of carbonic acid gas, the same quantity of water, under a double amount of pressure—two atmospheres, will absorb a double quantity of carbonic acid gas—22 volumes. Dalton has proved, that where there is a mixture of gases, the pressure of each individual gas alone determines the proportion in which it is absorbed by a liquid, the amount of its absorption being entirely independent of the pressure exerted by the surrounding gases.

Upon these premises, Vierordt and his followers endeavour to explain the mechanism by which the blood absorbs oxygen, and exhales carbonic acid. Let us suppose, for instance, the blood, on reaching the pulmonary capillaries, to contain a greater amount of carbonic acid gas than the carbonic acid present in the air-cells is able by its pressure to maintain in its condensed state in the liquid; a quantity of that gas, sufficient to establish the equilibrium, will at once escape from the blood. In other words, carbonic acid gas will be given off from the blood, until the amount present in that liquid is reduced to the quantity which pure blood is capable of absorbing under a pressure equal to that existing in the pulmonary air-vesicles. As regards the absorption of oxygen, the venous blood, on reaching the capillaries of the lungs, contains a much smaller amount of this gas, than pure blood would be capable of absorbing under a pressure equal to the one there experienced; and the blood will take up a volume of oxygen sufficient to supply the deficit. It is thus seen that the carbonic acid gas and the oxygen contained in the blood, on entering the

small vessels of the lungs, exist under circumstances exactly the reverse of each other; and that in order to restore their respective equilibriums, a directly opposite change supervenes in the case of each. One gives up the plus which it contained; the other receives the minus which it wanted. The volume of each gas is perfectly independent of that of the other, their interchange not being effected (according to a belief which still obtains to a certain extent) by a mutual displacement, but being determined by the respective amount of each gas present in the blood, and the pressure of the corresponding gas in the pulmonary air-vesicles.

We are unable at present to go more fully into the various theories promulgated of late years on this important and interesting subject, but shall devote the space at our command to the consideration of the mechanism of respiration and spirometry. We shall first call our readers' attention to the admirable treatise of Dr. Sibson, a work that bears the stamp of entire originality, while it conveys the assurance that the author is an acute and philosophical observer.

We propose successively to examine some of the more important points of his work. A few years before its publication, Dr. Sibson observed, while examining the chest in persons subject to dyspnœa, that the latissimus dorsi and the serratus magnus muscles acted during forcible expiration; and on further inquiry, he found that neither of them acted during inspiration. He also ascertained that in inspiration the scaleni acted the whole time; that the superior ribs approached each other, while the inferior moved farther apart; and that the *internal* intercostal muscles between the six superior costal cartilages, and the *external* intercostal muscles between the superior ribs, were in action. After having made an extensive series of observations and illustrations of the respiratory apparatus in animals breathing by ribs, he exposed the muscles of respiration in the dog and the ass while alive, and noted which muscles acted on inspiration, and which on expiration. He afterwards made the discovery, that when the lungs of a dead animal were inflated, the inspiratory muscles became shortened when the lungs were distended: an observation of the utmost importance, as it lessened the necessity for the repetition of vivisections.

Numerous observations on all classes of animals breathing by ribs, commencing with the simplest, and gradually ascending to the highest—the human organization—convinced our author that the mechanical portion of the respiratory process was far more complicated than is generally supposed; and that in some important particulars the commonly received opinions on the subject required to be essentially modified.

Dr. Sibson made his first observations upon reptiles of the serpent tribe which possess no sternum, their ribs being connected with the vertebræ only, and having no costal cartilages. In the snake, whose ribs, besides serving the respiratory movements, are also used for the purpose of progression, he found the internal intercostal muscles to be expiratory, and the external ones inspiratory; and by the aid of ingenious diagrams he has succeeded in explaining how, on inspiration, the ribs are raised, move forwards, glide on each other, and increase their intervening spaces. From the snake he passed on to the consideration of the respiratory function in

the chameleon, a cold-blooded animal, of sluggish movements, and therefore breathing slowly and irregularly, yet, nevertheless, requiring at times very deep inspirations, and very sudden and complete expirations. This animal was found by our author to possess a greater amount of rib-movement than almost any other creature; the addition of a complete system of anterior ribs or moveable cartilages affording it twice the power possessed by the snake for the expansion of its lungs. The inspiratory muscles acting on the ribs are the scalenus, the levatores costarum, and the external intercostals: they have the same action and nearly the same anatomical distribution as in the snake. Advancing in the scale of vertebrated animals, Dr. Sibson next transferred his investigations to the feathery tribe:

"The lungs in birds are embedded in the spaces between the ribs; they only have a free surface invested with the pleura anteriorly. The diaphragm in the fowl, forms with this free surface of the lung, an enclosed cavity.

"In mammalia, each lung is enclosed in a distinct pleural sac, the whole lung being free save where the air-tubes and great vessels are attached. This cavity is completely closed above, protected by ribs, muscles, and fasciæ. The first rib is more intimately connected than any other with the sternum; in the majority of animals it either directly articulates with that bone, or is united to it by a short, firm cartilage. It is in the expansion in every direction of the upper part of the chest, and the great range of descent of the whole diaphragm, the progressive lengthening of the ribs, the arched and comparatively mobile spinal column, and the usually small, jointed sternum, that the mechanism of breathing in the mammalia chiefly differs from that in birds; for in birds, the upper part of the chest is not closed in, the diaphragm has but a limited range of motion, the spinal ribs are nearly of a length, the spinal column is stiff, and the sternum is in one large piece." (par. 29.)

In mammals, the costal cartilages are analogous to the sternal ribs of birds. This is particularly observable in the sheep, the cow, and the pig, animals in which the costal cartilages are articulated with the ribs and the sternum by joints, differing from the sternal ribs of birds in the single circumstance of having an osseous in lieu of a cartilaginous structure.

"The relative proportion that the sterno-vertebral ribs, or those of thoracic respiration, bear to those of diaphragmatic respiration, depends on the size and form of the upper lobes of the lungs, compared with the lower.

"In the ass, the upper lobes are small and narrow, the lower are large and full at their posterior part. The lungs are short in front at the sternum, long and broad behind and below." (par. 31.)

Other animals have an exactly opposite conformation of chest. For example, in the dog, the body and the superior lobes of the lungs are large, and they are nearly as long anteriorly as posteriorly.

Dr. Sibson proceeds to discuss at considerable length the question of the costal mechanism; but our limits, and the impossibility of explaining ourselves clearly without the aid of diagrams, compel us to refer those of our readers who feel interested in the subject to the original memoir, and to pass on to review the muscles which act upon the ribs.

While in birds as well as in snakes, all the external intercostal muscles are inspiratory, and all the internal intercostal muscles expiratory; in mammalia, on the contrary, whose superior and inferior ribs have distinctly opposite motions, the superior intercostal muscles are observed to

have an opposite direction to the inferior. The action of the costal muscles being entirely subservient to the motion of the ribs, it is necessary to have a distinct comprehension of the movement of the latter before a just appreciation of the action of the former can be attained. For example: If the external intercostal muscle, situated between two superior ribs that approach each other, be inspiratory, the muscle having the same name and holding the corresponding relative position between two inferior ribs that recede from each other, will, on the contrary, be expiratory. Thus, in drawing a conclusion as to the action of any particular muscle, we must regard, not the system of muscles, but the system of ribs. Sometimes, however, it happens that the different fibres in the same muscle act in various directions. In the ass, our author observed on vivisection, that the superior fibres of the serratus magnus were expiratory, while the central ones supporting the scapula were neutral, and the inferior inspiratory; proving that, in one and the same muscle, the fibres may act in no less than three distinct directions.

The relative proportion of the respiratory apparatus to the size of the body differs exceedingly in different animals, and its magnitude is seen to depend principally on their habits of life. In the seal, the breathing apparatus is very large; the size of the animal's abdomen appearing quite insignificant in comparison with its capacious thorax. In the porpoise, this phenomenon is still more marked; for in addition to the usual thoracic and diaphragmatic space for breathing, a large portion of the lung occupies the neck: the porpoise is thus furnished not only with a thoracic, but also with a cervical respiration. Dr. Sibson attributes to the presence of this extensive breathing apparatus the power possessed by the creature of remaining a very long time under water. He conceives that it has the faculty of renewing at each inspiration nearly the whole volume of air contained in the lungs, the preceding expiration having almost entirely freed them from the adulterated air.

"The animal may descend under water with a far purer and far larger stock of air than land animals can, by any effort, obtain; of course, when the animal dives very deep, the quantity of air in the chest cannot be great." (par. 77.)

In man, the mechanism of respiration partakes, in one or more particulars, of the structure of each of the creatures already mentioned, and is consequently, in a certain degree, illustrated by their structure. The human chest is of ever-varying capacity. Its cavity can be enlarged simultaneously in every direction. The domed roof rises, the floor descends, and the diameter between the walls is increased on all sides. Here, however, varieties in the mechanism of respiration are also found to depend on age and sex. In the female, the development of the abdominal in relation to the thoracic cavity, is considerably greater than in the male; in the child, and still more in the fœtus, this distinction of size is peculiarly marked, from the great extent of the liver compared to that of the lungs. As age increases, the bulk of the pulmonary organs augments, in consequence of the gradual enlargement of the air-vesicles. Dr. Sibson notices that disease has a still greater influence on the respiratory movements; on this point he has truthfully remarked:

"If the larynx be narrowed, so as to permit but little air to enter the lungs, the diaphragm descends so rapidly that the air has not time to fill up those portions of the lungs displaced downwards by the diaphragm; the consequence is, that the pressure of the atmosphere forces backwards and inwards the costal walls on the lungs.

"If the summit of the lung be affected with phthisis, the corresponding portion of the chest is but little dilated, and the ribs are depressed and almost motionless over the diseased portion of the lung; sometimes the rib even falls in at the beginning of respiration.

"In inflammation of the lower lobe of the lung, that lobe is distended by diseased secretions; the chest over it is permanently expanded, and has little or no respiratory movement; at the same time, the upper portion of the same side of the chest is less actively inspiratory. If the diaphragm be inflamed on one side, that side does not act; the diaphragmatic ribs and the hypogastric region do not move forwards on the affected, though they do on the healthy, side.

"If any part contiguous to the ribs, on one side, should be injured by the respiratory motion of those ribs, then that side of the chest is often motionless, though the lung be sound.

"If the air-cells be dilated, the whole chest takes on permanently the form that it has on a deep inspiration.

"These instances show the practical value of a thorough knowledge of the healthy respiratory movements of each portion of the chest; attention is at once called to any point that, owing to disease, has not its due motion." (par. 101.)

This valuable paragraph introduces us gradually to the study of the vital capacity in health and disease, a study which has proved as beneficial as that of the mechanism of respiration to the science and progress of medicine.

In bringing our remarks on Dr. Sibson's paper to a conclusion, we cannot refrain from complimenting its author upon the able manner in which he has prosecuted his researches; to the physiologist they offer valuable suggestions as to the function of respiration, while they materially aid the practitioner in the diagnosis of disease.

The next point to which we shall direct our attention is one in which the scientific and practical are so intimately blended, that even the most empirical members of the profession can scarcely regard it without interest. The immediate result of Mr. (now Dr.) Hutchinson's labours to which we have to advert, was the discovery of a new physical means of diagnosis in pulmonary complaints, and one, moreover, of the utmost significance. The greatest credit is due to this diligent inquirer for having been the first to invent and to employ a philosophical instrument by which the quantity of air taken into the lungs at each inspiration can be accurately measured; and it speaks well for his practical acumen that he so nicely appreciated the true practical value of the researches he instituted, and was able, in the face of preconceived ideas, and numerous circumstances calculated to embarrass inquiry, to arrive at such just and important conclusions. Although some of his theories do not rest on so sure a basis as might have been desired, they were yet arranged and presented in a manner which could not fail to arrest the attention of the physiologist, and to furnish an impulse to further inquiry on a subject offering so magnificent a reward. This effect has in reality followed upon the publication of Dr. Hutchinson's work; since the time of its appearance many labourers in the field of scientific medicine have been induced to inquire

into the vital capacity of the chest; and from a repetition of our author's experiments, aided by others of their own invention, have been forced to unite their testimony with his in pointing out how a knowledge of the vital capacity of the lungs lends invaluable aid in the diagnosis of a disease most subtle at its commencement, and most fatal in its termination —pulmonary phthisis.

As early as 1679, the physiologist Borelli instituted an experimental inquiry into the quantity of air received into the lungs at each inspiration; and from his time to that of Hutchinson, numerous observers, among whom may be mentioned the names of Goodwyn, Davy, Thompson, Kentisch, Abernethy, Menzies, Kite, Allen, Pepys, Seguin, Herbst, and many others, attempted to measure the vital capacity of the lungs; but the instruments they employed and the results they obtained, alike failed to benefit to any great extent either practice or science. Dr. Hutchinson was the first to invent for this purpose what may be called a truly philosophical instrument; and by its use he was enabled to discover that in man the breathing capacity bears a certain relation to stature, and that this relation is much influenced by pulmonary diseases, especially tuberculosis. On account of the discovery of the changes produced upon the vital capacity in certain affections of the lungs, his observations acquired considerable practical value in the diagnosis of these complaints. Moreover, his researches led him to the conclusion that the arithmetical relation existing between the vital capacity of the thorax and the height of the individual, is but slightly altered either by the age or the weight of the body.

Dr. Hutchinson has divided his subject into the following heads:

"Firstly, the quantity of air expelled from the lungs, in connexion with other physical observations on the human frame.

"Secondly, the absolute capacity of the thorax with cubic superficial and longitudinal measurements.

"Thirdly, the respiratory movements and mobility of the chest.

"Fourthly, the inspiratory and expiratory muscular power.

"Fifthly, the elasticity of the ribs, and estimate of the voluntary respiratory power.

"Sixthly, the effect of the decussating, diametrical, and oblique power, in reference to the function of the intercostal muscles.

"Seventhly, general and practical deductions to detect disease by the spirometer, with the method of its application." (par. 15.)

Our author submitted to observation no less than 2130 persons, chosen from almost all classes of society—gentlemen and beggars, soldiers and sailors, giants and dwarfs, girls and pugilists, firemen and watermen, healthy and diseased; and in order to guard against the disturbance caused by adventitious circumstances, each individual was subjected to at least three observations, it being frequently found that, owing to the nervousness or awkwardness of the patient, the result of the first and second could not be deemed conclusive. Dr. Hutchinson found that the vital capacity of the lungs principally depended on three things—firstly, the height of the individual; secondly, the weight of the body; and thirdly, upon age. The first of these being the most important, as we shall have occasion to point out.

The knowledge of the vital capacity of the chest has been further

extended by the researches of Davies, Green, Walshe, and Pereira in England; Fabius and Schneevogt in Holland; Vogel, Simon, Haeser, Albers, Stellwag, Küchenmeister, and Wintrich in Germany; and lastly, by the important work just published by Arnold, which we shall notice in conjunction with that of Hutchinson. Although all the writers agree upon general principles, they are at variance upon several important details; by comparing their researches and conclusions, we hope to present to our readers a tolerably correct idea of the prevailing views regarding the points at issue.

No single individual can perfect any particular department of science. Far from being astonished, therefore, at the inability, even of the lucid mind of Hutchinson, completely to master his subject, we are, on the contrary, inclined to marvel at the number of difficulties which yielded to his inquiries. Viewed in this light, the following remark from his pen will not be unexpected:

"We have seen," he says, "that the amount of air taken into the lungs at each inspiration corresponds with the height and not with the absolute capacity of the thorax. Why is this the case? I confess myself as much at a loss to explain it as I was the first day I commenced the research. I believe the vital capacity is commensurate with the range of mobility or thoracic movement; but why the mobility increases in arithmetical progression with the height, which appears chiefly dependent on the length of the limbs, and not on the length of the trunk of the body, I am completely incapable of explaining. So completely are mobility and vital capacity affected by the stature, that a man will breathe in different positions different quantities of air. Thus, standing, I blow 260 cubic inches; sitting, 255.; and when recumbent (supine), 230; prone, 220: position making a difference of forty cubic inches." (par. 117, p. 196.)

We shall soon see how subsequent observers, and more especially Arnold, have been able to explain paradoxes by which Hutchinson acknowledges himself to have been puzzled.

Besides inventing an instrument to measure the vital capacity of the chest, Hutchinson constructed another to test the inspiratory and expiratory power, which varies greatly in different individuals. This last-mentioned instrument, composed of a simple curved tube, containing a column of mercury as a resistance against the respiratory power, enabled him to demonstrate that, in men standing five feet seven or eight inches, the inspiratory power is at its maximum; and from this height, contrary to what might have been anticipated, strength gradually decreases with the increase of stature. Thus, on an average, men of five feet seven or eight inches, elevate a column of mercury three inches, while men of six feet cannot raise it more than two and a half inches. We see that the researches of Dr. Hutchinson were of two distinct kinds. One was directed to the measurement of the absolute volume of air given out by a forced expiration; the other, to testing the relative strength of the respiratory power. In a practical point of view, the former series of researches is most useful in private practice, the latter in selecting men for military and naval service.

We cannot give a more striking example of the diagnostic value of the measurement of the breathing capacity by the spirometer, than by quoting the case of Freeman, the American giant:

"This man came over to England in 1842, and, in the November of that year, trained for a prize fight. I examined him immediately before his *professional engagement*, when he might be considered in the 'best condition.' His powers were as follows:—Vital capacity, 434 cubic inches; height, 6 feet 11½ inches; weight, 19 stone 5 lbs.; circumference of his chest, 47 inches; inspiratory power, 5·0 inches; expiratory power, 6·5 inches. In November, 1844, exactly two years afterwards, he came to town in ill-health. I then examined him in the same way as before, twenty times, at various intervals, during which his vital capacity varied from 390 down to 340, and the mean of all the observations was 344 cubic inches —a decrease of 90, or more than 20 per cent. His respiratory power had decreased one-fifth, and his weight 2 stone in the whole period. At this time I took him to two physicians well skilled in auscultation, and they both affirmed that they *could not detect* any organic disease. After January, 1845, I lost sight of Freeman; and in the October following, I was kindly favoured with the following account of him from Mr. Paul, surgeon to the County Hospital, Winchester:

"'Freeman was admitted into this hospital on the 8th of October, in an extreme state of debility and exhaustion; he was reduced almost to a skeleton, complained of cough, and was expectorating pus in large quantities. Percussion on the anterior part of the chest, *under the clavicles*, gave on the right side a very dull sound; on the left one, much clearer, but still, I think, less resonant than natural. I made but one attempt at auscultation, but could come to no conclusion, for a rather singular reason—the ribs were so large, the intercostal spaces so wide, and so sunk in from the extreme state of emaciation to which Freeman was reduced, that I could not find a level space large enough to receive the end of the stethoscope—could not, in short, bring its whole surface into contact with the chest. Freeman's great debility, and the clearness of the diagnosis from other sources, prevented my repeating the attempt. Freeman after death measured 6 feet 7½ inches, and weighed 10 stone 1 lb. On opening the chest, the lungs on both sides were found adhering by their apices to the superior boundaries of the thorax, and studded throughout their substance with tubercles. The tubercles, on the whole, were much less numerous in the right lung than in the left; both lungs were nearly healthy at their base; the tubercular matter gradually increased in quantity towards their upper part, and the apices of both lungs were almost completely occupied by large cavities partly filled with pus, and capable of containing two or three ounces of fluid each."' (pars. 168, 169.)

This one case is sufficient of itself to show the importance of the spirometer in detecting the incipient stage of pulmonary disease, at a period when the other physical means of diagnosis are incompetent to the task. We may add a few words upon its use, and upon the results that have been obtained by its agency.

We have already drawn attention to the opinion of Hutchinson, that the vital capacity of a healthy individual depends principally upon stature, weight, and age, and is most powerfully influenced by the first. So intimate indeed is the connexion existing between the stature of the body and the capacity of the thorax, that for every inch of height (from five to six feet) eight additional cubic inches of air, at a temperature of 60° Fahrenheit, are given out by a forced expiration. The second element, weight, is of minor importance, and cannot be so easily estimated, inasmuch as the weight of an individual increases with the height. In order to obtain the true relation between the weight of the body and the vital capacity of the lungs, Hutchinson first attempted to fix the average normal weight for a given stature, and then proceeded to compare the result with the amount of air expelled by a forced expiration. Numerous observations on this point led him to the conclusion,

"That the vital capacity increases nearly in the ratio of one cubic inch per pound from 105 to 155 lbs; and that from 155 lbs. to 200 lbs. this increase is overpowered, and there is a loss of thirty-nine and a half cubic inches, as the effect of weight. Therefore all weight under eleven stone and a half does not interfere with the vital capacity; but, on the contrary, it increases with the weight up to this point; but above this weight, so far as our table goes (namely, to fourteen stone), the weight interferes with the vital capacity, preventing this increasing progression in the relation of rather more than one cubic inch to the pound. . . . . The weight of man naturally increases with the stature, therefore the relation between the weight and the vital capacity must also vary at different heights." (pars. 55, 56.)

Our author found the influence of the third element, age, on the vital capacity, to be less than that of either of the preceding ones; and concluded, from extended observations on individuals at different periods of life, that from the fifteenth to the thirty-fifth year of age it increases; decreasing, on the other had, in the progression of from nineteen, eleven, and thirteen cubic inches, from the thirty-fifth to the sixty-fifth year.

According to Hutchinson, the circumference and length of the chest have little or no influence in regulating the amount of air taken in at each inspiration; first, because the circumference of the thorax increases with the weight of the body in exact arithmetical progression of one inch for every ten pounds; secondly, because the length of the chest, according to his observations, varies but slightly, the stature of a human being depending on the length of the legs, not upon that of the trunk.

"One man of six feet and half an inch standing, sat only two feet eleven and three-eighth inches; while another of five feet six inches standing, sat three feet high; and therefore the standing height does not appear to correspond with the sitting height, or the length of the body with the length of the trunk." (par. 90.)

The mobility of the chest, Hutchinson, on the other hand, ascertained to have a powerful influence over the vital capacity; the mobility was calculated by a double measurement of the circumference of the chest, immediately above the nipples, with an ordinary tape measure: firstly, during a deep inspiration; secondly, after a full expiration; the difference between the two observations giving the mobility of the thoracic walls. This was observed to vary considerably in different persons; and from a number of examinations he educed the average mobility, in people of middle stature and weight, to be about three inches, seldom four.

These conclusions, at which Dr. Hutchinson arrived after many careful, and oftentimes complicated, inquiries, have been for the most part corroborated by subsequent observers.

Professor Arnold has availed himself in an admirable manner of the published researches of Hutchinson and others of his predecessors. The labours of this accomplished physiologist are fraught with a peculiar value. He employed and compared the various methods adopted by Hutchinson, Fabius, Wintrich, and other observers, and drew out a series of extensive and most useful tables from the results of their calculations and his own. He estimated the average vital capacity of the lungs, for every additional inch of stature, length of trunk, and circumference of chest; tested the influence of weight at the different heights, and fixed the average thoracic mobility at each stature, in order to arrive at a just appreciation of the influence exerted by size, weight, length of

trunk, circumference and mobility of chest, on the vital capacity. He also pointed out the influence of the mode of life, of social position, of age, sex, and other differences in the bodily state, so that he may be said to have given to the spirometer a far greater practical value than it before possessed. A single glance at Arnold's tables will indicate to the practitioner what ought to be the normal vital capacity of the lungs in any given case, and enable him to state whether his patient breathes a greater or less amount of air than the average of individuals under similar circumstances.

Simon has for the most part confirmed Hutchinson's views. He also found that the vital capacity bears a direct relation to the height of the individual, increasing with the increase of stature, in a fixed scale. According to Simon's observations, the advance in the vital capacity from a stature of 156 centimètres (62·4 inches) to 180 centimètres (72 inches), is 1350 cubic centimètres (86·4 cubic inches). This gives for every $2\frac{1}{2}$ additional centimètres (1 inch) in a person's height, an addition of 150 cubic centimètres (9·6 cubic inches) in the vital capacity, 19 6 cubic centimètres (1·2544 cubic inch) more than were found by Hutchinson. Simon and J. Vogel endeavour to explain this discrepancy, by asserting that the experiments of Hutchinson, being made chiefly upon men of very strong constitutions, yielded something more than the average individual capacity. Simon's own experiments had been made upon students between the ages of seventeen and twenty-five, whose constitutions he supposes to have neither more nor less than an average strength. Arnold, in his tables, while steering a middle course between Hutchinson and Simon, nevertheless, in his calculations, gives to the former observer the credit of superior accuracy, asserting Simon's results to be far below the average.

Fabius boldly opposed the assertion made by Hutchinson and Simon, that the vital capacity stands in direct arithmetical relation to the height of the body. The fact already discovered by Simon and Hutchinson, that in some cases the vital capacity cannot be subjected to the rules laid down for its measurement, is asserted by Fabius to be in itself a direct proof of the uselessness of endeavouring to establish a graduated scale between stature and breathing capacity. He even goes so far as to declare, that if any one would measure the ears of a thousand men, and try to discover the relation existing between the size of the ears and the capacity of the thorax, the results would not prove less satisfactory. He states that we must seek in the length of the trunk and the mobility of the thoracic walls, an answer to the question, "What is the vital capacity?" We might, he says, judge *à priori* of the vital capacity of the lungs from the capacity of the thorax; for the greater the circumference and length of the chest, the more will the pulmonary organs be able to expand, downwards as well as laterally; and the greater the mobility of its walls, the greater will be the capacity of the thorax. It will be necessary, therefore, before the vital capacity of a man can be theoretically determined, to measure the length, circumference, and mobility of the chest. As the measurement of the length of the chest is attended with difficulty, he proposes, starting with the idea that the thorax invariably constitutes a fixed portion of the trunk, to measure the latter.

As the chest, however, has not the form of a perfect cylinder, and does not expand in a fixed ratio, the formula according to which the capacity of a cylinder would be found, cannot be applied to it, but a more universal one must be sought.

It appears to us that Fabius has fallen into just as great an error as the one he condemns: when asserting the difficulty of measuring the length of the thorax, he proposes as a substitute the measurement of the trunk, falsely pre-supposing that the thorax bears a distinct and unvarying relation to the abdominal cavity. In two *perfectly formed* persons of equal stature, the size of the thorax will in both cases be the same; and this size will not only be proportional to the whole length of their bodies, but also to the length of their trunks; so that whether the capacity of the chest be calculated in proportion to the stature or to the length of the trunk, the result will in either case be the same. This exact symmetry of body is, however, rarely, we might say scarcely ever, found, and least of all among civilized nations, where occupation, habits of life, and social position have so variously influenced the development of the human frame. In some persons, we find an excessive development of the limbs; in others, of the trunk. In some the cavity of the chest is long and narrow; in others, short and broad. But as, for a given size of body, a fixed amount of air must be necessary for the maintenance of perfect health, the cavity receiving that air must have a normal cubic capacity, so that if it is long, it may be proportionately narrow, and what it wants in length, it may make up in breadth. We should therefore consider it wisest, supposing such a course practicable, to leave all measurements of stature and length of trunk alike out of the question, the inquirer confining himself to the dimensions of the thorax alone. This is the opinion held by Professor Arnold. We shall have occasion to recur to it.

Finally, Fabius inferred from his experiments, that the vital capacity depended upon four things. Firstly, on the length of a man's trunk (imagining the length of the thorax to be always in proportion to that of the trunk); secondly, upon the circumference of the chest at the nipples; thirdly, on the mobility of the thoracic walls; fourthly and lastly, on the age. He agrees with Albers* in the statement, that strong men respire more than weak ones, and thinks, with Hutchinson, that the position of the body has a great influence on the quantity of air expelled from the lungs by a forced expiration. He holds, with Küchenmeister, that the vital capacity of the female is not at all diminished at the time of pregnancy.

We cannot refrain from here noticing the valuable paper published a short time since by Wintrich,† and will briefly sum up the result of his observations, which derive great importance from having been made upon no fewer than 3000 males and 500 females, of all ages between the ages of six and ninety. This learned observer has paid particular attention to the influence of stature, age, sex, position of body, effects of food and drink, state of bowels, of pregnancy, and rapidity of respiration. From his researches, he concluded that no importance is to be attached to the weight of the body, or even to its height, unless taken in conjunction with the age.

* Medicinische Wochenschrift, Sept. 1852.

† Handbuch der speciellen Pathologie und Therapie, Band v. Abtheil. 1.

According to Wintrich, the breathing capacity is at its maximum between the ages of twenty and forty; and this is exactly what theory would lead us to anticipate, seeing that every organ requires for the performance of its function a certain amount of oxygen; and as the quantity of oxygen must increase or diminish with the increasing or diminishing activity, we should naturally expect that the greatest quantity would be required for the system at the period when the frame has reached its most perfect state of development, with every function in most active operation. As the lungs are the principal organs through which this interchange of gases takes places, it is natural to conclude that during the prime of manhood, with every function of the animal body at its climax, that of respiration would likewise be at its maximum.

We have already mentioned the importance attached by Wintrich to age, in connexion with vital capacity; and that he should do so will not excite surprise when we glance at the result of this observer's calculations. In children of both sexes, between the ages of six and eight, he found only between 6·5 centimètres (2 6 inches) and 9 centimètres (3·6 inches) of expired air to every centimètre of stature. Between the ages of eight and ten years the proportion is from 9 cent. (3 6 in.) to 11 cent. (4·4 in.) of expired air to every centimètre of stature; from ten to twelve years it averages 11 cent. (4·4 in.) to 13 cent. (5·2 in.), to 1 cent. ($\frac{2}{5}$ths of an inch) of height. Between twelve and fourteen years the proportion is from 13 cent. (5·2 in.) to 15 cent. (6 in.), to 1 cent. ($\frac{2}{5}$ths of an inch) of stature. Wintrich made few observations on persons beyond the age of fifteen; but the few experiments he instituted had very noteworthy results. Between the fortieth and fiftieth year of life no very appreciable variation in the vital capacity occurs in either sex; between the fiftieth and sixtieth years, however, an important diminution is observed, induced by two causes—the commencement of atrophy of the lungs, and the obesity frequently accompanying this period of life. From the sixtieth to the ninetieth year a great decrease in the vital capacity takes place; but it cannot be reduced to an arithmetical scale.

Various observers had endeavoured to test the value of age in connexion with vital capacity, without arriving at any satisfactory result; the reason of this failure being, that their examinations were confined to persons in the middle period of life. Arnold has reduced the researches of Hutchinson to the following tabular form:

"From the twentieth to the thirty-fifth year there is an increase in the vital capacity of 135 cubic centimètres, in the following proportion:—

"From the	20th	to the	25th	year,	increase of	10	cubic cent.	(·64	cubic inches).	
,,	25th	,,	30th	,,	,,	28	,,	(1·792	,,	).
,,	30th	,,	35th	,,	,,	97	,,	(6·208	,,	).

"From the thirty-fifth to the sixty-fifth year there is a decrease of 888 cubic centimètres in the amount of air expired. The proportion is as follows:—

"From the	35th	to the	40th	year,	decrease of	266	cubic cent.	(17·024	cubic in.).	
	40th	,,	45th	,,	,,	172	,,	(11·008	,,	).
	45th	,,	50th	,,	,,	78	,,	(4·992	,,	).
	50th	,,	55th	,,	,,	64	,,	(4·096	,,	).
	55th	,,	60th	,,	,,	181	,,	(11·584	,,	).
	60th	,,	65th	,,	,,	127	,,	(8·128	,,	).

(Arnold, p. 79.)

Here it is seen that, in the 1st period after the twentieth year, the increase is smaller than in the 2nd; and in the 3rd it is considerably greater than in either of the previous ones. On the other hand, the decrease in the 1st period is by far the greatest; in the 2nd it is less; and in the 3rd and 4th smaller still; while in the 5th and 6th it again becomes more marked.

It is not a little amusing to note the difference of the conclusions arrived at by various observers on the same subject. Two observers, even, reasoning on the same fact, offer various, and not unfrequently opposite, explanations.

Wintrich, we perceive, states as his firm belief that age has a most important influence over the vital capacity; and that in the examination of all cases, especially of persons not yet arrived at puberty, and also of those in the decline of life, this influence must be taken into most careful consideration.

After having learned the opinions of other observers on the same subject, this assertion of Wintrich's will at first sight appear somewhat surprising; a moment's thought, however, will explain the apparent anomaly, if we bear in mind that Hutchinson, Simon, and the rest, who attached no importance to the age of the individual under examination, drew their conclusions exclusively from the observation of persons in middle life; while Wintrich went to the very extremes of early and latter age, at which periods alone the influence of age in increasing or diminishing the vital capacity is so definite as to admit of no doubt. His observations are scarcely sufficiently numerous to enable us to estimate with perfect accuracy the effect of age upon the amount of respired air, but they are sufficiently marked to arrest the attention of future inquirers.

The vital capacity of females has been observed to be somewhat less than that of males of equal stature, the average of this diminution being from 6 to 6½ cub. cent. This difference is even smaller than we might have been led to anticipate, when we consider the various modes of life of the two classes. It has been always found that people of sedentary habits respire less than those leading an active out-door life; and the occupations of women are, we know, considerably less active and much more sedentary than the generality of avocations pursued by men. Arnold, moreover, adduces, in addition, the following physical causes as explanatory of this diminished vital capacity:— Firstly, the relation between the depth of the chest and the length of the body; secondly, the relation existing between the thoracic cavity and its external circumference being less in women, in consequence of the size of the mammary glands and the surrounding fatty deposit; thirdly, the diminution in the mobility of the thoracic walls, consequent upon the female mode of dress; and this last cause plays a most important part, for the same female, relieved of her corset, will respire from 100 to 200 cub. cent. more than when encased in her stays.

The smaller vital capacity of women, in comparison with men, may be said, therefore, to depend upon the four following points:—Firstly, the relation between the stature and the length of the thorax; secondly, the small circumference of the internal thoracic cavity when compared to

that of its external surface; thirdly, the diminished thoracic mobility; fourthly, the mode of life.

While we are on the subject of mode of life, in its influence upon the breathing capacity, it may not be superfluous to remark that Arnold's tables demonstrate very distinctly the alteration produced by occupation and mode of life in the vital capacity. He found, for example, that in general the upper classes, students, and paupers (persons not subjected to bodily exertion) have a smaller vital capacity than the labouring classes, such as mechanics, printers, firemen, &c. On the other hand, sailors, soldiers, and recruits have a higher breathing capability than either of the preceding classes. So great is the diversity in these three classes, and so important does our author consider it, that in the calculation of the vital capacity he recommends particular attention to be paid to the class of which the person under observation is a member. If he is to be ranked in the first class, 100 cubic centimètres (6·4 cubic inches) should be deducted from the average amount of vital capacity as given in the tables; if to the second class, 100 cubic centimètres (6·4 cubic inches) must be added; and should he be a member of the third class (sailors, &c.), an addition of no less than 300 cubic centimètres (19·2 cubic inches) is to be made. The low vital capacity of the upper classes and students doubtless depends upon their sedentary mode of life, and the small amount of muscular labour they undergo. The favourable effects of constant employment in the open air are, on the other hand, no less clearly demonstrated in the great vital capacity of sailors and soldiers. The mechanic, again, who, though actively employed in bodily labour, is subjected to a certain amount of confinement, occupies, as might have been expected, a middle position between the other two classes, in respect to vital capacity. The very small breathing power possessed by paupers appears to arise from their having insufficient material to support the respiratory process, it being a well-known fact that in cases of inanition not only the quantity of oxygen absorbed and of carbonic acid exhaled, but even the number of respirations, are notably diminished, the very first day after insufficient nourishment has been taken.*

We have already spoken at some length of the effect of stature on the vital capacity. We must now notice the results of a few of Arnold's very complete series of experiments. Some of these the writer of this article had the privilege of witnessing; he can therefore testify to the care and exactitude with which they were made. These experiments had for their object the appreciation of the relative value of the measurements of stature as asserted by Hutchinson, over the vital capacity in comparison with the measurement of the trunk *per se*, as instituted by Fabius; and the observer came to the conclusion that, although neither of his predecessors was entirely wrong, yet neither was exactly right. Indeed, he believes, as we have already mentioned, that inasmuch as it is the extent of the thoracic cavity alone which regulates the quantity of air the chest is capable of containing, it would be far better to put aside entirely all measurement of the length of the trunk and the height of the person, and consider simply the dimensions of the thorax *per se*. The only measurement of the thorax attended with any difficulty is that of its length;

* Bidder und Schmidt, Verdauungssäfte, § 370. Leipzic, 1852.

but this difficulty may be almost obviated by a measurement of the length of the sternum, which will give a tolerably accurate idea of that of the thorax. The circumference of the chest, which is easily taken, Arnold, in common with Buys, Ballot, and Möbius, considers an important factor in estimating its capacity—i.e., when the increase of circumference is not due to an excessive muscular development, an abnormal deposit of fat, or other diseased condition. For wherever he found an increase of 5, 10, 15 centimètres (2·, 4·, 6·, inches) in the circumference of the thorax, in healthy individuals of similar age, height, and weight, there resulted an increase of 100 cub. cent. (6·4 cub. in.), or even more, of vital capacity. He therefore entirely differs in opinion from Hutchinson and Donders, who imagine that measurements of the chest, so far as regards circumference, may be left entirely out of the question in the calculation of vital capacity.

The third point in the measurement of the chest, and the only one upon which all observers coincide, is its mobility or expansibility; Hutchinson was the first to point out its agency. He found, as might have been *à priori* expected, that the more the walls of the chest expand, the greater is the volume of air they can contain. This is in a remarkable degree demonstrated in Arnold's tables. If the mobility or expansibility of the chest be increased from 3 to 11 cent. (1·2 to 4·4 in.), the vital capacity receives an augmentation of about 1256 cub. cent. (80·384 cub. in.). The increase is thus seen to be very marked; and it will be observed that it increases in as regular an arithmetical progression as is found either by height or circumference.

The next observation of Arnold's which in the progress of our subject it behoves us to notice, is one made in connexion with the weight. His views on this point come into collision with those of the plurality of observers. The relation between the weight of the body and the vital capacity can only, he opines, be said to exist in as far as an increase of weight generally accompanies an increase of height. The weight of the body, independently viewed, cannot be invested with any importance as bearing upon vital capacity; for with the same weight of body the vital capacity increases with the height, while with similar heights it does *not* perceptibly increase with the weight. On the contrary, the inquirer is not unfrequently staggered by the fact that a man weighing several pounds more than another of equal stature, possesses a more limited capacity; so that we often see the lighter individual respiring the greater amount of air.

We have already had occasion to indicate the circumstance, that the vital capacity is influenced to a considerable extent, even in persons with a perfectly-formed chest, and who are entirely free from pulmonary complaints, by the various states of the system and its organs. In illustration of this assertion, Arnold relates an interesting case of the effect of hypertrophy of the liver in a youth aged twenty, whose stature was 175 cent. (70 in.), with a thoracic circumference of 82 cent. (32·8 in.), and whose volume of expired air was reduced from 3750 cub. cent. (240 cub. in.) to 2561 cub. cent. (163·904 cub. in.), apparently from the effects of this hepatic disease. At the time of the first examination, the liver of this youth was visibly enlarged, and the epigastrium painful to the touch; while a year later,

the patient respired 3300 cub. cent. (211·2 cub. in.), the cardiac region being much less painful, and the liver considerably diminished in size. No doubt, as the disease continued to abate, the vital capacity would proportionately augment; it is to be regretted, however, that as the patient did not again present himself for inspection, this could not be proved by a renewed examination. The state of the stomach is equally important in its effect upon the breathing capacity; a hearty meal is not unfrequently found to have the power of reducing the vital capacity of the lungs to the extent of 100 to 200 cub. cent. (6·4 to 12·8 cub. in.) from its former volume. In further illustration of this principle, we may quote the experience of Fabius, who tells us that his servant respired no less than 250 additional cub. cent. (16 cub. in.) after having his bowels well purged by the administration of a very powerful cathartic. The tendency of all these facts is to point out the necessity of taking closely into consideration the condition of the abdominal organs and their contents before we can expect to acquire an accurate knowledge of the state of the thoracic cavity by the employment of the spirometer. It is remarkable that, while an hypertrophied liver, a distended stomach, or a costive bowel, exerts such an influence, the gravid uterus, even in the latter months of pregnancy, when it fills up and distends the abdomen to an enormous extent, is said to manifest no action whatever over the vital capacity. This apparent anomaly can only be explained by the supposition, that as the uterus enlarges, the abdominal cavity extends in a corresponding ratio; so that towards the end of pregnancy, the space in the abdomen unoccupied by the gravid organ is not less in extent, although different in form, than in the unimpregnated state. Arnold has attempted to explain this condition by supposing—firstly, that in females, the extension of the thoracic cavity takes place more in the upper part than in males; and secondly, the decrease of the mobility of the diaphragm from above downwards, consequent upon the enlargement of the uterus, being compensated by the increase in the diameter of the base of the thorax, so that the minus in the length of the chest is counterbalanced by the plus in its breadth. This alleged independence of the vital capacity with regard to pregnancy requires further investigation, as not one of the explanations hitherto given of the fact (if fact it be) can be considered satisfactory.

Our limits, however, forbid our devoting more space to the spirometer; we must therefore now confine ourselves to the enumeration of the circumstances to be regarded in calculating the vital capacity of a healthy person. They are—firstly, the stature; secondly, the circumference of the chest; thirdly, the mobility of the thoracic walls; fourthly, the age; fifthly, the position and occupation; and sixthly, the sex.

To facilitate the calculation of these points in determining the vital capacity in any individual, with a view of judging of his state of health by the comparison of his respiratory powers with those of the average of healthy persons of a similar physical conformation, Professor Arnold has carefully prepared two tables—one for men, another for women, as the vital capacity in the latter sex is somewhat smaller in relation to physical measurement than in males. In these tables, the average normal vital capacity can at a glance be found for any stature between 154 and 191 cent. (61·6 and 76·4 in.), and any circumference of chest between 65 and 100 cent. (26

and 40 in.). It is now an established fact that, in men of equal stature, the vital capacity may vary considerably; and that where the mobility is equal, the difference is principally regulated by the difference in the circumference of the chest. Thus, for instance, a man of five feet eight inches in stature, with four inches mobility of chest, and a circumference of thirty inches, possesses a smaller vital capacity than a man of similar stature and thoracic mobility and thirty-four inches of circumference. The operator has therefore to measure his patient, refer to the tables, and add the quantity given for the circumference to that found for the stature, and divide by 2, in order to discover the normal vital capacity of the person under observation. As a great muscular development or a deposit of fat, however, makes a difference in the circumference of the chest of from two to eight inches, and the before-mentioned tables are calculated at an average development in determining the vital capacity of a man of spare habit, two inches ought to be added to his circumference, and in one of excessive development from two to eight deducted, according as the case may require. In females of very spare habit of body, no less than four inches are to be added to the circumference; and in those of excessive development, from two to four deducted from the found circumference of chest. Another point, and one of perhaps still greater importance, must not be passed unnoticed. This is the thoracic mobility, which modifies to a great degree the quantity of air respired, whether this quantity exceed or fall below the average of persons taken as a healthy standard. The mobility of the chest, though it cannot be said to increase in any marked degree with the circumference, is observed, on the other hand, to progress regularly with the height of the body. Arnold has explained this fact by assuming that, in tall persons, the distances between the ribs are greater than in those of a less stature; while in persons of the same height, even where the thoracic circumference is very different, the width of the intercostal spaces remains the same. The wider the intercostal spaces, the greater, *cæteris paribus*, must be the mobility of the chest. It is probable, in this relation, that we may trace the reason why the mobility of the thoracic walls increases with the stature of the body, and not with the thoracic mobility. The proportion of this increase is seen in the following table:

"Stature	157—160	cent.	(62·8—64 in.),	thoracic mobility	6·5	cent.	(2·6 in.)
„	161—165	„	(64·4—66 „)	„	6·5	„	(2·6 „)
„	166—170	„	(66·4—68 „)	„	6·85	„	(2·7 „)
„	171—175	„	(68·4—70 „)	„	7·49	„	(2·99 „)
„	176—180	„	(70·4—72 „)	„	7·80	„	(3·1 „)
„	181—185	„	(72·4—74 „)	„	8·30	„	(3·3 „)
„	186—191	„	(74·4—76·4 „)	„	8	„	(3·2 „)

"Here the mobility of the thorax is seen to increase with the stature, and had the observations been further extended, the progression would doubtless have been found much more regular. In round numbers, the following may be supposed to be the average relation between stature and mobility of chest.

"Stature	157—165	cent.	(62·8—66 in.),	thoracic mobility	6·5	cent.	(2·6 in.)
„	165—170	„	(66 —68 „)	„	7·0	„	(2·8 „)
„	171—175	„	(68·4—70 „)	„	7·5	„	(3·0 „)
„	176—180	„	(70·4—72 „)	„	8·0	„	(3·2 „)
„	181—191	„	(72·4—76·4 „)	„	8·5	„	(3·6 „)"

(Arnold, p. 70.)

The value of this factor (thoracic mobility) in calculating the vital capacity, has been distinctly pointed out by Arnold. He has, for example, shown, that if a man of 166 cent. (5ft. $6\frac{1}{2}$ in) in height, with a thoracic circumference of 80 cent. (32 in.), having a mobility of 7 cent. (3 in.), respires 3420 cub. cent. (21·88 cub. in.), being the average amount given in the tables; another man of the same height, and with the same circumference of chest, but with a mobility of 8 (2·3 in.), will respire 180 cub. cent. (11·55 cub. in.) more, or 3600 (230·4 cub. in.). If he have a mobility of 9 cent., which is 2 in excess, he will respire twice 180 cub. cent. (11·55 cub. in.) more, or 3780 cub. cent. (241·92 cub. in.). On the other hand, if instead of an excessive mobility, we have a diminution, as for instance when, with a similar stature and circumference, the mobility is 6 cent. (2·4 in.), instead of 7 cent. (3 in.), the value of 1 cent. ($\frac{2}{5}$ in.) of mobility (= 180) (11·35 cub. in.) is to be deducted from the normal amount of air respired. Ex.: 3420 — 180 = 3240 cub. cent.

It is unnecessary to repeat the remarks already made with regard to the calculations necessary for a determination of the vital capacity of persons belonging to the various classes of society, which have already been indicated as exercising a material influence over the quantity of air respired. When the volume of air emitted into the spirometer by a forced expiration is found to be from $\frac{1}{10}$th to $\frac{1}{6}$th less than the physiological average, as set down in the tables, a diseased state of the pulmonary organs can with every probability be assumed, and the abnormal condition is most probably one of tubercular disease. Indeed, one need scarcely dread giving such a diagnosis, even where neither the stethoscope nor percussion is sufficiently acute to detect the least sign; for, as has been already said, the spirometer is a physical means far more delicate than either in the detection of this complaint. Wintrich, who perhaps has had greater opportunity than any other inquirer for examining the vital capacity in persons suffering from pulmonary disease, considers the spirometer a more valuable means of detecting phthisis in its earlier stages, than either auscultation or percussion; and Schneevogt, moreover, corroborates his further assertion, that he could diagnose tuberculosis by the spirometer, when every other means had failed. Hutchinson has delivered as his opinion that, in the first stage of tuberculosis, the diminution in the vital capacity averages from $\frac{1}{10}$th to $\frac{1}{2}$, and in the second stage from $\frac{1}{6}$th to $\frac{1}{3}$, of the normal amount.

In other pulmonary complaints, especially in their acute stage, the spirometer can scarcely be said to render efficient service; for while acute inflammation is present, each effort at forcible expiration induces an attack of coughing, which completely destroys the exactitude of the observation. The following facts, however, gathered by Arnold, deserve notice:

"Firstly. In slight acute bronchitis the vital capacity diminishes from $\frac{1}{15}$th to $\frac{1}{10}$th, and in chronic from $\frac{1}{8}$th to $\frac{1}{3}$rd below its normal condition. Secondly. Immediately after pneumonia or pleurisy, the vital capacity is diminished from $\frac{1}{4}$th to $\frac{1}{5}$th. Thirdly. In cases of exudation into the pleural sac, accompanied with compression of one of the lungs, the vital capacity sinks from $\frac{1}{3}$ to $\frac{3}{4}$ below the physiological average. Fourthly. Emphysema reduces the amount of air expired from 11 to 60 p. c., but this can be traced in a great measure to the very small thoracic mobility always accompanying those cases." (Arnold, p. 131.)

We shall now proceed to enumerate, in regular sequence, the results we have endeavoured in the foregoing pages to explain. We must from these data allow that the spirometer possesses not only a scientific, but likewise a practical, value.

Apart from practical use, the spirometer has rendered efficient service, in a scientific point of view, in revealing to us the following interesting and important moments:

1. The vital capacity of man increases with his stature, in the proportion of 150 cubic cent. (9·6 cub. in.) for every $2\frac{1}{2}$ cent. (1 in.) increase in stature. If, therefore, a man of 155 cent. (62 in.) stature, possesses an average vital capacity of 2700 cub. cent. (172·8 cub. in.), another of 170 cent. (68 in.) stature will have a breathing capacity of 3600 cub. cent. (230·4 cub. in.), and a third person, 180 cent. (72 in.) in height, a capacity of 4200 cub. cent. (288·8 cub. in.).

2. In men, the vital capacity increases with the circumference of the chest, in the same ratio—namely, 150 cub. cent. (9·6 cub. in.) for every $2\frac{1}{2}$ cent. (1 in.) increase in thoracic circumference. If the chest measures 65 cub. cent. (26 in.), the vital capacity will, on an average, amount to 2580 cub. cent. (165·12 in.). With a thoracic circumference of 80 cent. (32 in.), it will increase to 3480 cub. cent. (222·72 cub. in.); and if the circumference be as much as 90 cent. (36 in.), the vital capacity will be no less than 4080 cub. cent (261·12 cub. in.).

3. The mobility of the chest has a great influence over the vital capacity. It increases with the stature, from 157 (62 8) to 190 cent. (76 in.), about 2 cent. (·8 in.)—i.e., from 6·5 cent. (2·6 in.) to 8·5 cent. (3·4 in.). Its value, however, augments with the circumferencs of the thorax, in a proportion of 160 cub. cent. (10·24 cub. in.) for every cent. of increased mobility, with a circumference of 75 cent. (30 in.). Where there are 80 cent. (32 in.) of circumference, the additional volume of inspired air is 180 cub. cent. (11·52 cub. in.); and where the thoracic circumference is respectively 85 cent. (34 in.) and 90 cent. (36 in.), the increase of the vital capacity is respectively 210 and 240 cub. cent. (13·24 and 15 36 cub. in.).

4. The vital capacity increases from the fifteenth to the thirty-fifth year of age nearly 160 cub. cent. (10·2 cub. in.), and sinks from the thirty-fifth to the sixty-fifth year about 900 cub. cent. (57·5 cub. in.), at different periods, and in different proportions.

5. The position, occupation, and mode of life, have an undeniable influence on the breathing capacity. At its minimum among paupers and in the higher classes, the vital capacity is highest in sailors, soldiers, and strong young men with out-door occupations, such as recruits, for instance; and finds its medium in mechanics, compositors, and pressmen.

6. In women, the vital capacity is absolutely and relatively less than in men. It increases in the female sex at the rate of 100 cub. cent. (6·4 cub. in.) for every additional $2\frac{1}{2}$ cent. (1 inch) in height, and with the circumference of the chest in similar proportion. Its medium amount in women of 144 cent. (58·4 in.) stature, is 2000 cub. cent. (128 cub. in.). At a stature of 154 cent. (61·6 in.), it averages 2200 cub. cent. (140·8 cub. in.); at 164 (65·6), 2800 cub. cent. (179·2 cub. in.). With a thoracic circumference of 71 cent. (28·4 in.), the vital capacity amounts to 1900 cub. cent. (284 cub. in.); with 81 (32·4 in.) circumference, 2300 cub. cent. (147·2 cub. in.); and with 91 cent. (36·4 in.) cir-

cumference, the vital capacity measures 2700 cub. cent. (127.8 cub. in.). (Arnold, p. 156.)

Not only is the spirometer, as we have shown, a most important help in the diagnosing of certain pulmonary complaints, and consequently exceedingly useful in practice, and a valuable means of ascertaining the eligibleness of men for military and naval service; but it is also a very efficient agent in calculating the probability of longevity for insurance companies. We were not a little surprised when we learnt, on inquiry a short time since, that many of the metropolitan insurance offices had abandoned the use of the spirometer, alleging as the reason of its rejection, that the indications obtained from its employment were very frequently fallacious, and little to be depended on. The small satisfaction derived from the use of the spirometer, cannot, however, be traced to any inadequacy of the instrument itself as a means of calculating the vital capacity, but to the fallacious principles on which the calculations were based, and the absence of reliable tables giving the average physiological capacity. The improvement in the mode of calculating the vital capacity, and the possession of the very complete tables so carefully drawn up by Arnold, for gauging the vital capacity of any individual, must now be considered as having removed the cause for these objections; and we therefore confidently recommend the re-introduction of the spirometer, as a means of testing the chances of life, feeling assured, as we do, that if used on the principles laid down by Arnold, its employment will entail neither unsatisfactory nor fallacious results.

In cases where artificial respiration is required, the good effects of the means employed have not unfrequently been completely counteracted by excess in the volume of air forced into the lungs to resuscitate the asphyxiated person. The delicate air-vesicles of the lungs having been ruptured during the operation, emphysema supervenes, and the patient is not unfrequently rescued from impending suffocation, to become a martyr to a disease which may ultimately prove fatal. An accurate knowledge of the volume of air which the lungs of any individual are capable of containing, is therefore exceedingly important in regulating the application of instruments for artificial respiration; and this knowledge is easily obtained by a measurement of the chest, on the principles of Arnold, already cited in the preceding pages of this article. This knowledge, moreover, is indispensable to the proper application of the beautiful instrument for artificial respiration, invented by Dr. Marcet, which is so constructed as to regulate to a nicety the amount of air admitted into the lungs at each inspiration. We have frequently watched with extreme interest the application of Dr. Marcet's instrument, in the reanimation of dogs purposely asphyxiated, and have been struck by the exactitude with which the volume of air was each time proportioned to the size of the animal, and with the great success which attended the experiments performed. Judging from the success of this gentleman's attempts in the restoration of animals, we should expect an equally favourable result from the employment of Dr. Marcet's artificial respirator in cases of asphyxia occurring in the human subject. We have thus specially called attention to this instrument, from the fact that the highest advantage it is capable of affording, is only to be reaped when the operator avails himself of the information furnished by Arnold's tables.

In concluding our remarks on the interesting labours of Hutchinson, Arnold, and others, it now only remains for us to thank these observers for the great amount of practical as well as scientific information they have afforded us, and to express the hope that, ere long, they may be enabled to remove the uncertainties which still hang around a few points of this important subject. So much has already been achieved, that we are sanguine in our anticipations of seeing the remaining difficulties solved by these philosophical inquirers.

George Harley.

Review VI.

On Unsoundness of Mind, in its Medical and Legal Considerations. By J. W. Hume Williams, M.D.—*London*, 1856. pp. 238.

The human mind, whether in health or in disease, must ever form one of the most interesting and important subjects for meditation and inquiry which can occupy the attention of the physician. The study is necessarily difficult, and the more so because we can scarcely be said to have arrived at any *positive* knowledge on the subject; and if this is true as regards the mind in a healthy state, it is still more so in its diseased conditions—when, in fact, there is such a departure from its normal manifestations as constitute what we call mental unsoundness. Any contribution to our knowledge on a subject so perplexing deserves our thoughtful attention; and we cannot but express our conviction that Dr. Williams has rendered a great service to the profession and to society at large by the very able and lucid manner in which he has discussed this deeply-interesting question. We heartily hope that his book will be generally read, not only in our own, but in the legal profession, where the most mistaken views are adopted, and a degree of knowledge assumed which could only be the result of personal experience among the insane; and this we know falls to the lot of but very few except members of the medical profession. Our author says:

"Counsel acquire their ideas of soundness or unsoundness of mind as some do their notions of special affections, from nosological books which lay down their fixed descriptions of disease. Physicians may, on examination, admit the general truth of the one, and allow the accuracy of the other. Who is there, however, who has stood by the bedside of the sick, and seen the student of the closet, but has felt that the most important part of his knowledge was wanting, or the capability of applying the information he had acquired? The lawyer is this student of the closet! It would be quite as rational to expect that the jury, if guided by his opinion on the soundness or unsoundness of mind in a particular case, would place equal reliance on his advice respecting their individual states of health, from detailing to him certain symptoms, whose value as indications of various diseases, nosological works have with equal confidence laid down. This is a proposition, we are satisfied, to which few would assent; for in their own cases they would ignore the competency of counsel to estimate the practical application of a science which they feel satisfied must be studied in the great volume of nature, written in works, not words." (p. 27.)

In considering the various degrees of departure from the standard of mental health, our author points out the importance of being properly informed as to the natural standard in the particular individual under

examination. It must ever be borne in mind that this standard is as variable as are the temperament, constitutions, and idiosyncrasies of individuals; and that if we would form correct judgments of their mental states, we must carefully ascertain what has been their natural standard of mental health, and then estimate the apparent departure from it as evidenced by conduct, ideas, and general bearing.

Reference is made to the notorious inconsistency of the law as propounded by the judges, who maintain that an offender is punishable, insane though he be, if it can only be shown that he knows right from wrong—a degree of intelligence undoubtedly possessed by a large majority of insane persons. While we fully concur in the principle that an insane person cannot properly be held responsible for acts committed under the influence of his malady, we yet feel bound to dissent from the doctrine advocated by our learned author, of absolute irresponsibility in every case of mental unsoundness. If insanity be a disease, surely it may differ in its intensity; and according to this differing intensity, it appears reasonable that we should estimate the amount of responsibility *quoad* crime. We do not pretend that the most acute and experienced observer can accurately define the precise mental condition of an individual with a view to fixing the amount of responsibility which may be properly attributed to him; and therefore we conceive that wherever *any* degree of mental unsoundness is proved, considerable allowance should be made for a certain other degree, which possibly is not susceptible of proof, but may nevertheless exist. But while fully recognising the principle that unsoundness of mind may properly be pleaded in extenuation of crime, we yet conceive that it is a dangerous doctrine to hold that any trifling amount of mental unsoundness is to be admitted as an excuse for any amount of crime. It argues nothing against the unity of the mental principle that it may suffer *partial* disturbance, and eventuate in partial irresponsibility. An absolute loss of control over the passions and actions of the individual appears by no means a necessary sequence of a minor degree of mental unsoundness. This power of control may surely be impaired without being lost; the mainspring may be weakened without being broken. An individual may be conscious of a morbid inclination to crime—morbid as something opposed to his natural disposition, and resulting from disease—which he may yet have the power to restrain, but which he may take no pains to check. Insomuch as we have reason to believe that he had the power to impose this restraint upon his inclination, and failed to do so, to that extent we hold that he was responsible; and, while we readily admit the extreme difficulty of determining the degree of his helplessness, and would therefore make large allowance, we are not prepared to hold him entirely guiltless. We must not forget that society has its rights as well as individuals, and these are not lightly to be ignored in dealing with a class of offenders of all others the most dangerous, because presumed to enjoy an absolute immunity from punishment. Where there exists any degree of mental unsoundness, we fully concur in the views entertained by our author on the subject of capital punishment, and hold that such an exhibition would be a national disgrace; but we are by no means so satisfied that secondary punishments may not be very properly inflicted, if it were only to warn those who are

disposed to presume upon their eccentricities, which may even be assumed, to serve a purpose of revenge. It is impossible to say to what extent the wicked though pretended attempts on the life of the Queen might have been carried, if the absurdity of acquitting on the ground of insanity the vain and ignorant perpetrators of these atrocities had been persisted in. The notoriety, however infamous, was a distinction eagerly sought by such degraded beings, and constituted a powerful incentive to the crime. In some, at least, it is probable that there existed a degree of mental unsoundness, but the wisdom of treating them as criminals notwithstanding, cannot be questioned. The punishment cured the mania, the warning deterred others, and this class of criminals has ceased from among us.

The sentence of penal servitude for life in the recent case of Westron for the murder of Mr. Waugh, marks the commencement of a new era in our criminal legislation of the highest importance, and recognises a principle which we conceive to be both sound and politic. By such a course we cannot commit any serious amount of individual injustice; for while we give to society the protection which such a warning is calculated to afford, and which it unquestionably has a right to claim, we are yet in a position to transfer the offender to an asylum if his mental condition be found really to require such care and treatment.

We regret to find ourselves at issue with our author on this question of partial insanity, a subject, however, which may receive considerable elucidation by impartial and unprejudiced discussion. We therefore strongly advocate the attentive perusal of the work before us, with the conviction that it will amply repay the reader for the time bestowed upon it.

Review VII.

1. *Actstykker angaaende Cholera — navnlig, Epidemien i Christiania i* 1850.

Documents relating to Cholera—viz., the Epidemic in Christiania in 1850.—*Christiania,* 1851. 12mo. pp. 128.

2. *Actstykker angaaende Cholera-Epidemien i Norge i* 1853.

Documents relating to the Cholera Epidemic in Norway in 1853.—*Christiania,* 1854. pp. 199, 106.

3. *Norsk Magazin for Lægevidenskaben.* 1849.

Norwegian Magazine for Medical Science. 1849.

Two years ago we drew the attention of the readers of this Journal to the important Report on Cholera in Sweden, drawn up by Professor Berg, of Stockholm. In the review of that Report, the bearing of the documents collected in Sweden on the great question of the contagion or non-contagion of this disease was prominently brought forward and discussed. We have now the opportunity of performing the like task in reference to the sister kingdom of Norway, a country which in many respects presents equal, if not greater, facilities for studying the mode of propagation of this malady.. In proportion to its population, Norway has a more extensive seaboard, and a more lively commerce with foreign countries, and the question of contagion in reference to quarantine regulations is,

therefore, in the latter country, even of more vital interest than in Sweden.

The documents now before us only refer to the epidemics of cholera as they occurred in Norway in 1850 and 1853; but we have also availed ourselves of some most interesting reports on cholera in Bergen, on the west coast of Norway, in 1847, contained in the 'Norwegian Medical Journal' * for 1849. We have not as yet received the reports of the last epidemic in Sweden, in 1853, or of the terrible invasion of cholera at Copenhagen in the summer of the same year.

The report on cholera in Norway in 1850 is drawn up by the Royal Central Cholera Commission; and that of 1853 by the Medical Committee of Christiania. Both are well compiled; both bear the strong impress of that honesty and impartial search after truth so characteristic of our Norwegian brethren.

In its general features, the cholera of Norway does not differ in the slightest degree from the characters it has presented in other countries which it has traversed. It were useless, therefore, to go over symptoms already sufficiently known; it would be a waste of time and of our readers' patience to detail the pathology and the course of the malady, or to relate the various attempted modes of cure. Each of the last named occasionally succeeded, and as often failed; and here, as elsewhere, it was only towards the close of the epidemic, when its virulence appeared to be subsiding, that remedies seemed to be of much avail. We shall subsequently notice the treatment that found most favour in Norway; but our chief object, in the present instance, is to continue the investigation we commenced in our article on Cholera in Sweden,† and to ascertain how far, in Norway, the progress of the malady tends to corroborate or refute the opinions we there expressed.

When cholera first appeared in this country, in 1831, we acknowledge that we then held, and for many years after continued to hold, opinions opposite to those we now entertain on the question of the contagion of the disease. In 1831, the majority, we may say, of the European practitioners were decided contagionists; but subsequently to that first invasion of the disease a reaction of opinion occurred, and the question was virulently discussed for years without any definite conclusion. In 1848, when the malady again appeared amongst us, many of the higher authorities coincided with the solemn declaration of the Board of Health, that the malady was not in any way contagious, and that no danger was incurred by attendance on the sick. The experience of that year, however, in the town where we now write, led us to an opposite conclusion; and this change of opinion was still further confirmed by what we observed during the last severe invasion of cholera in Newcastle-on-Tyne, in September, 1853.

It seems to us that the tide of opinion is again now in favour of contagion, modified perhaps, and scarcely so exclusive as that doctrine was held by some in 1831, but still contagion or infection—and we shall use these two words simply to express the propagation of the disease from one person to another. We leave it, however, to our readers to determine

* Norsk Magazin for Lægevidenskaben. † See No. for Jan. 1854.

how far these new documents bear upon this question; we shall lay the facts before them, and draw our own conclusions.

Before proceeding to examine the progress of cholera in the East of Norway, we shall turn our attention to two reports in the 'Norwegian Medical Journal' before alluded to, and which appear to us to contain details of much value in reference to the subject.

Cholera first appeared in Norway in 1832, when it broke out, in the month of October, at Drammen. Christiania, which is about thirty miles distant, then escaped; but in the following year (1833) the disease again appeared at Drammen, spread to Christiania, and from thence progressed along the east side of the Skagerack, in the direction of Sweden. Nor did the towns on the western coast of that inlet then escape altogether; but the western coast of Norway remained still wholly free. In 1831, cholera existed in the ports of the White Sea as far as Kola; but it did not extend to Norway, though vessels in which fatal cases had occurred rode quarantine in Hammerfest harbour. Bergen remained free from the epidemic till the winter of 1848, when the first case appeared on the 11th of December. The disease continued to prevail in the town and neighbourhood till the 10th of April, 1849, when the last case was reported. A very full and careful description of the disease is given by Candidatus Medicinæ, T. J. Löberg, who was attached to the Lazareth, or Cholera Hospital, in the town. Bergen contained at that time nearly 24,000 inhabitants: of these, 1024 were attacked by the disease, and 605 of those so attacked fell victims to the pestilence. Of the general symptoms of the malady, as given by Löberg, it is needless here to speak; the character of the disease was the same as in other countries, and the results of treatment not more satisfactory, as is evidenced by the mortality having been nearly 60 per cent. of those attacked. Löberg, with many of his countrymen, is inclined to regard the constant vomiting, which in this country we have so generally striven to arrest, as a favourable symptom, or as an effort of nature which should be assisted rather than opposed. But while we thus summarily dismiss the careful records of the symptoms of cholera afforded us by Kierulf and Löberg, we wish to express our entire satisfaction with the conscientious and diligent manner in which they have performed their task. It is no fault of theirs if their researches, their post-mortem examinations, their microscopical and chemical investigations of the fluids and solids in cholera, have led to no positive results; for may not the same be said of many, if not all, of the bulky volumes and reports which have from time to time been issued since the first invasion of this pestilence? At some future time no doubt all these researches will be of value, when the master mind shall arise to elicit from the mass of reliable documents and records the true nature, pathology, and perhaps even the appropriate treatment, of the disease. Löberg and Kierulf both avow themselves to be contagionists, after the experience they have had of the progress of the disease in Bergen and the adjacent country.

"I would not allude more to the question of contagion," says Löberg, "were it not that the observations I made during this last epidemic have forced me to draw conclusions opposed to those of the majority of writers on the disease. To form a just idea of the spread of the disease by contagion, one must have practised as a

cholera physician; and the facts thus brought before us soon show that the doctrine of the propagation of the disease, drawn as it has generally been from observations made in large towns, is incomplete and hardly to be relied upon. Such is not the case, however, in smaller communities, and particularly in country districts, where the epidemic advances slowly, and each new case attracts immediate attention. Here the propagation of the disease can often be followed in its minutest details.

"The first case of Asiatic cholera appeared in Bergen on the 11th of December, in the house of a watchman, who lived in the greatest poverty. No communication with any infected person or locality could be discovered. The wife and a child of the watchman were first affected, and both died; after that the husband was attacked; and on the seventh day another child of the same family, which had been removed into another house three days before. The wife's mother, who had for a time attended her daughter, lived in one of the poor-houses of the town, the so-called Asylum; and from this place, when she sickened, she was taken to the hospital. Immediately the disease spread through the Asylum, where many individuals resided in each room. It ran from bed to bed and from chamber to chamber, always attacking those who had attended the sick before they were removed to the Cholera Hospital. In this way, not less than twenty individuals were carried off by the disease in this house (the Asylum) before the 22nd of December; up to which time the malady had not spread in the rest of the town, excepting to one house adjoining the watchman's, where two people were attacked on the 18th of December, and to the Sailors' Poor-house, which lies only one hundred paces from the Asylum. Between these two houses there was undoubtedly intercourse, and the first case in the Sailors' Poor-house occurred on the 17th of December. Here the disease ran exactly the same course as it had done in the Asylum, always affecting those who had attended those attacked before them. In the meantime, a hospital was fitted-up in (Christi Krybbe) 'Christ's Manger,' to which the necessary fittings were gradually brought, after patients had already been placed therein, by men from the workhouse; and in this latter place the disease showed itself on the 25th of December. By this time it had begun to spread through the town, so that it was difficult to follow each individual case; but this much is certain, that the malady advanced only step by step, so that the disorder was at its height in some portions of the town while other parts remained perfectly free. The malady first raged in the few houses wherein it first appeared, and then progressed from quarter to quarter without overleaping any considerable space until it reached Nordnaes, the extreme point of the town towards the north, situated on a peninsula, and far distant from the opposite side of the town where it had commenced. Whole families were carried off in the early part of the invasion; and of the 300 sick in the hospital at 'Christi Krybbe,' two-thirds at least stated that they had been in communication with cholera patients, either to nurse them, or to put on leeches, or to lay out the dead. Then the disproportionately great number of the attendants on the hospital who were affected deserves notice; for of these, not less than one physician, eight porters (*portoger*), four nurses (?) (*gangkoner*), the cook and her assistant, and afterwards a second girl in the kitchen, several bearers of the dead, two men who took away the straw of the mattresses on which the patients had lain, two who washed for the hospital, and one who carried out the dirty linen, &c., were affected at one time or other with the disease. It seems to me, therefore, that it is impossible to deny here the operation of contagion. To me it is clear that the disease produces *a volatile infection* (*et flygtigt smittestof*). [?] We believe that we observed certain circumstances tending to show that this miasma is not always confined to the immediate neighbourhood of the sick person. In proof of this we may remark, that the malady appeared in certain houses wherein some of the hospital attendants resided, but who themselves remained free from the disease; and in other cases, communication of the disease took place from infected houses by the means of individuals who themselves escaped altogether. Indeed, I can state for certain, that three of

these cases were the very first cases of the disease in the respective quarters of the town in which they appeared, so that it would be difficult to affirm them to be the results of an epidemic influence which had not as yet reached these districts. One example is too striking to be passed over:—While cholera was for the most part confined to the Asylum and the Sailors' Poor-house, a woman took ill of the disease in the latter establishment, and was attended there by her sister. This sister lived in Nordnaes, a quarter of the town which, as I have before stated, was not reached by the malady till several weeks after, for this occurrence took place in the first ten or twelve days of the invasion of cholera. Two days after this sister had left the Sailors' Poor-house and had returned home, a fatal case of cholera occurred in her house in Nordnaes, in the person of a female with whom she resided; and for several weeks this was the only case of cholera in that part of the town." (p. 298.)

The alterations of temperature that took place during the prevalence of the disease were, as might be expected from the season of the year, very considerable. The climate of Bergen is notoriously bad and changeable at all times; and even in the depth of winter, from the proximity of the ocean, there is not that steady, uniform cold temperature which prevails in Sweden and Russia at the same latitude, or even much farther to the south. A few days before the cholera appeared, the thermometer stood at + 11° Reaumur. but a day or two after it sank to several degrees of cold, and then the temperature continued to change, often very suddenly, fogs alternating with clear weather, followed by snow or rain; but all these changes seem to have had little influence on the disease. Nor did the winter's storms seem to affect the progress of the malady. During the last three months of the epidemic, furious winds from different quarters frequently prevailed, but the malady crept on apace, undisturbed by the tempest. We have generally seen that cholera is most severe during close, still weather, such as prevailed so remarkably in this town (Newcastle-on-Tyne) during the severe epidemic of September. 1853; and we believe that a rapid movement of the atmosphere, by dispersing the particles of the miasm, and not permitting it to gain strength by accumulating in any particular spot, is a most effectual bar to the progress of the disease. In 1853, after a fortnight of the stillest weather imaginable, during which the cholera raged fearfully in Newcastle, affecting the higher and better parts of the town to an equal if not to a greater degree than the poorer and lower situations, a sudden abatement of the malady was observed to follow on the occurrence of the equinoctial gales; and the disease never regained its previous intensity. In Bergen, this was not the case; but the habits of the people may possibly account for this difference. In these high northern latitudes, great care is taken to exclude in winter the access of cold air from without; the air within the rooms is seldom renewed, and thus it becomes charged with the miasm to a high degree.

The opinions and experience of C. T. Kierulf upon this subject will be best learned by a short *résumé* of his report on cholera in the vicinity of Bergen, in the same year. It is extracted from the 'Norse Medical Magazine' for 1849, vol. iii. part 8.

It will be seen from this report that Kierulf admits the existence of a certain miasm in the air in cities affected with cholera. We do not think it is possible to deny that such is the case; and we assert, too, that the same miasm floats in the atmosphere when epidemic invasions of

small-pox or of scarlet fever desolate any crowded community. But we differ from many others in regarding this volatile miasm as the product of the bodies of the sick: we do not believe that it is transported to a great distance, except it be carried by individuals. Of telluric influences we know nothing; of atmospheric influence we admit only a condition of the air highly favourable to the concentration of the poison, and such a condition we believe to exist where the air is still and warm, such as it has so frequently been observed to be during the severe invasions of cholera. Under such circumstances the air of a city, being not renewed by fresh currents from the country, becomes tainted with the miasm from the bodies of the sick, like the air of a room that is not ventilated; and in this way perhaps we can explain, in part at least, the rapid extension of the disease during hot weather. Other conditions of the atmosphere, as yet very imperfectly known, probably influence the spread of cholera, such as the amount of ozone, and the relative electrical conditions, which are known to vary so considerably. We do not, however, think that any of these are of themselves sufficient to engender cholera; they only, in our opinion, act as carriers of the miasm, or as a fruitful soil in which the seeds of the poison, once introduced, will rapidly multiply.

Kierulf observes that in the thickly-populated towns and districts of France and Germany the majority of medical men are (or at least were) opposed to the idea of contagion; while it is upheld more or less by the medical authorities of Denmark, Sweden, and Norway. In the last-named country, he remarks, there are but two or three absolute contagionists, while almost all the others admit that the disease is occasionally propagated in this manner, though they deny that contagion is the sole means, or even the most frequent one, of its extension. He (Kierulf) formerly was a decided opponent of the doctrine of contagion, but his experience during the epidemic in and around Bergen induced him to adopt the contrary opinion.

"From the isolation of the dwellings, and the difficulty of communication on the western coast of this country (Bergen district), it follows that the cases of cholera were more widely spread, and it was more easy to trace the propagation of the disease from hamlet to hamlet. As an example of this, I shall take the district of Fane parish, to which I was attached as a cholera physician during the greater part of the epidemic. This parish lies partly inland and partly on the sea coast, a few miles from Bergen; the nearest part being about three miles and a half, the most distant about fourteen miles from that town. In this district there occurred twenty-three cases of cholera, and in regard to each of these I was able distinctly to ascertain from whence each individual had received the infection, nor was there a single instance among these in which it was necessary to refer to the obscure theory of a miasm to explain its origin.

"Cholera broke out in Bergen on the 11th of December, 1848, and the last case was reported on the 10th of April, 1849. The disease showed itself late in January among the fishermen who had been in Bergen, and from thence had gone to the fishing stations south of that town, and from thence, and from Bergen, all the inhabitants of Fane parish seemed to have received the infection.

1. Andreas Isachsen, Haaland, aged twenty-two, had been in Bergen on the 23rd of December, and had slept in a house there where that day no one was sick, but wherein on the following day two fatal cases of cholera occurred. He took cholera on the 25th, and died on the 28th. 2. Ole Larsen Oppedal, of Hope, aged twenty, took ill on the 30th of January, and recovered by the 12th of

February. He had been in Bergen two days before he was attacked. On the 6th—10th of February, three of the inmates of the farm-house at Hope were attacked with cholerine. 3. Andreas Larsen, Röe, aged thirty-six, took ill at the fishing-station on the 5th of February, was brought home on the 9th, and died the same day. He was buried on the 13th, and of those who attended his funeral, several persons, who lived at two farm-houses a considerable distance from Röe, were affected on the 19th with cholerine. 4. The widow of the last-named patient, Dorthe Hansdatter, aged thirty-five, took cholera on the 13th of February, and recovered on the 18th. Their children escaped the disease. 5. Malene Thomasdatter, Bredvig, aged twenty-seven, had been at the burial of No. 3 at Röe, on the 13th of February, and took ill of cholera on the 14th, and recovered by the 23rd of that month. 6. On the night of the 24th, her husband, Lars Andersen, Bredvig, aged thirty-five, occupied the same bed with his convalescent wife. During her illness he had slept with the children in another bed in the same room. He took ill on the 24th, and died on the 26th, of February. 7. Elling Nielson, Krogeide, aged fifty-seven, had slept at Bredvig, and had nursed No. 6 in that house on the 25th and 26th of February. He took ill on the morning of the 27th, and died the same afternoon. 8. His wife, Kari Johnsdatter, aged fifty-two, took ill on the 2nd of March, and recovered on the 9th. On the 1st of March their three children took diarrhœa, as did also No. 9, the sister of Elling Nielsen, Martha Nielsdatter, aged sixty-two, who lived in a house close to her brother's, and had visited him during his sickness. In this patient cholera was fully developed on the 5th of March, and she died on the 9th. 10. Ole Pedersen, Espeland, aged sixty-six, was brought home dead from the fishery on the 15th of February, having taken ill there on the 12th. He was buried on the 20th, and the funeral banquet was attended by both men and women (*'der blev holdt Gravöl af Mænd og Qvinder'*). 11. Lars Olsen, Espeland, aged thirty-eight, had accompanied his father's corpse from the fishery, and had attended him while sick. He took ill on the 16th of February, and recovered on the 28th. 12. Malene Andersdatter, Espeland, aged fifty-six, who lived in the next room to Lars (11), and had attended upon him, took ill on the 19th of February, and died in twelve hours. 13. Christiane Myntevig, aged sixty, had attended the *Wake* (Gravöllet) at Espeland, on the 20th; she took ill that day, and died on the 22nd of February. 14. Frederik Arnesen, Espeland, aged twenty, came home ill with Nos. 10 and 11, on the 16th of February, but cholera did not develop itself completely till after the 20th, and he died on the 7th of March. 15. Johannes Olsen, Sövig, aged fifty-two, was in Bergen on the 14th of February, and died on the 16th, after six hours' illness. 16. Johannes Michelsen, Bratland, aged thirty, had been in Bergen on the 24th of February, and had been in a house where cholera then prevailed. He took ill on the 25th, in the afternoon, and recovered by the 3rd of March. 17. Lars Larsen, Hamremyren, aged one year, was attacked with cholera on the 24th of February, and recovered by the 10th of March. The servant at Hamremyren, Malene Sjursdatter, aged thirty-six, was several times in the week at Bergen to sell milk; she was in that town, for instance, on the 17th, 19th, and the 22nd of February, and lodged there in a house where cholera prevailed, and wherein, on one of these days, an aged milk-woman died of cholera. On the 22nd, Malene herself took ill in Bergen, and was taken to the Lazaretto in Christi Krybbe. Lars Larsen, her master, father of No. 17, and twenty years of age, took cholerine on the 18th, but recovered by the 26th of February. 18. Jane Monsdatter, aged thirty-six, mother to No. 17, took cholera on the 27th of February, and died on the 3rd of March. 19. Her father, Mons Pedersen, aged sixty-six, who lived with his son-in-law, took ill on the 28th of February, and died on the 2nd of March. His wife escaped the disease. 20. Jacob Aadland, aged forty, was taken ill on the 27th of February, after having called at Espeland two days before on his way home from the fishery. He died on the 28th. 21. Kari Larsdatter, aged forty-seven, took ill on the 28th of February, and died on the 1st of March. Her husband had returned a few days before from the fishery, in company with No. 20,

but was not himself affected. 22. Lars Olai Larsen, of Söreide, aged thirteen, took cholera on the 3rd of March, and recovered on the 13th. His foster father, Tollev Nielsen, aged fifty-eight, belonged to the same fishing station as the men from Espeland, Aadland, and Hope, and had returned from thence on the 23rd of February, suffering from choleraic dysentery, which abated eight days after, but was not finally cured till the 12th of March. 23. The sister of the last-named, Martha Nielsdatter, aged 60, who inhabited the same room, took cholera on the 4th of March, and died on the 8th." (p. 579).

Kierulf observes that these facts speak for themselves; they show that cholera may be propagated by contagion, and that in general from one to four days elapse from the supposed period of infection to the outbreak of the disease. Most frequently the disease appeared on the second day after exposure to infection. It is probable, too, he remarks, that persons affected only with the choleraic diarrhœa can infect others with true cholera. Of this we have no doubt whatever, for we have always considered the diarrhœa so frequent during the invasion of cholera, to be a part and parcel of the disease itself. Kierulf believes, too, that even those who escape the disease themselves may convey it in their clothes to others. The report of the Royal College of Physicians seems to favour this opinion: and in 1853, we met with one or two cases where this seemed certainly to be the case. We have reason to think that scarlet fever is conveyed in this manner, and we see no cause why this mode of transport should be denied. An example of this kind is given in Kierulf's report.

"Sjur Nielsen, Soltvedt, of Hammer parish, aged forty, was engaged at the fishery, on board of a yacht, which for a time lay moored to a small island on which several cholera bodies lay unburied. A few days after, four men took ill on board of the yacht, two of whom died on the 11th and 12th of February; the other two were on that day brought on shore. No more cases of cholera appeared on board the vessel. On the 20th, Sjur Nielsen came home, and as he was afraid of communicating infection to his family, he changed his clothes before he went into his dwelling. On the following day he went to Bergen, from which place he returned in the afternoon of the 26th February. But already on the morning of the 26th, his wife, and before mid-day, his eldest daughter and youngest son, were attacked with cholera, and the latter died the next morning; while his eldest son took cholerine on the 28th. His family lived in an isolated house, far from any neighbours, and had had no communication with any other persons. There was then no cholera in the vicinity, and indeed hardly any in the whole district." (p. 580.)

Kierulf witnessed several instances where the malady seemed to have been communicated through the medium of clothes, bed-coverings, and the straw on which cholera patients had lain. One singular case occurred where five persons in one house partook of milk which had been brought from a dwelling where cholera prevailed. The milk in question had stood on a shelf over a bed wherein a cholera patient lay. Those persons in the same house who did not partake of the milk, escaped the disease. The fishing stations around Bergen are mostly placed on dry and sterile rocks, and therefore are not placed in conditions supposed to be particularly favourable to the development of cholera.

We now turn to the two volumes of reports alluded to at the commencement of this notice.

The Report of 1850 is in the modest form of a pamphlet of about 130

pages. Appended to this is a map of the town of Christiania, showing the extension of the disease in that capital. We find in this little volume most carefully-arranged details of the progress of cholera in Christiania, in two reports drawn up by C. L. R. Horbye, and by the well-known Professor Frederick Holst, of Christiania. We would gladly have extracted at some length from these reports, but after a careful perusal of their contents, we do not find that they afford any new lights on the symptoms or on the treatment of the disease. Here, as in all other instances, the earliest cases were generally the most intractable; and it was only when the virulence of the malady was subsiding, that it appeared to become more amenable to treatment. More attention has evidently been paid by our Norwegian brethren to the post-mortem appearances in cholera than has generally been done in this country; but we must confess that only negative results have been obtained.

Following these two able reports, which occupy about 80 pages out of the 160, we have an interesting series of remarks by Professor Faye, of Christiania, on the mode of propagation of cholera, as illustrated by the epidemic of 1850 in Christiania. Professor Faye discusses the whole subject with much fairness and candour; he acknowledges that under certain circumstances the disease exhibits contagious properties, but he is not prepared to admit of contagion as the sole, or even a frequent, means of spreading the disease.

Cholera first visited Christiania in 1833, and extended from thence towards Sweden, along the eastern coast of the Christiania Fiord. The disease seems never to have penetrated into the country, but to have been confined almost entirely to the towns along the coast of Norway. The roads and the great lines of communication throughout the interior are very few, yet in each sequestered dale, rarely visited even by the exploring tourist, hamlets and isolated dwellings are to be met with. These almost entirely escaped, while cholera continued to follow along the sea coast, where active communications are constantly kept up both by sea and land. If the disease were conveyed by atmospheric or telluric influences, we can hardly understand how the inhabitants of the interior should have so uniformly escaped, and how the miasm should have crept along the shores alone. True it is that in the larger seaport towns the sanitary condition of the lower classes is here, as in other countries, eminently favourable to the development of disease; but in the cottages of the Norwegian peasants of the interior, a lamentable want of cleanliness and neglect of ventilation are but too apparent to the traveller.

In 1850, cholera broke out at Christiania in the night of the 4th of October, in the suburb called Grönland. During the months of July, August, and September of that year, diarrhœa, cramps, and vomiting had been frequent; but as such complaints are of annual recurrence at that season of the year, they did not excite especial notice. At this time, cholera prevailed at Götheborg (Gottenburg), in Sweden, and southwards, along the coast of that country, while many of the Baltic towns on the Prussian side and elsewhere, as Lubeck, &c., were affected with the disease.

The first patient was a tinman, residing in the suburb of Grönland, a man of somewhat dissipated habits, but who had not, as far as is known,

been in communication with any suspected persons or localities. Cholera was still far distant, it had not approached nearer than Gottenburg, and some of the islands about the mouth of the Götha Elv (Gotha river). It had existed in Malmö, a town at the south-western point of Sweden, since the middle of August. No interruption, however, seems to have been offered to the steam-boat communication with the infected ports, beyond a short quarantine of five days; but as this included the period occupied by the voyage, it proved no great hindrance to traffic. The epidemic was confined to the suburbs of Christiania on this occasion; only three cases occurred in the town itself, and these three were in the general hospital. The duration of the disease was eight weeks—viz., from the 4th of October to the 28th of November, during which time there were 157 cases and 87 deaths. The population of the city and suburbs in 1845 was 31,703. Not only was the disease confined to the suburbs, but to a few streets in those districts; while other streets, equally objectionable in a sanitary point of view, and in the immediate vicinity of those affected, entirely escaped. Not less than 138 cases out of 157 occurred in some low-lying streets and houses near the Aker river, which streets were wretchedly paved, and were traversed by a small brook, which was the receptacle for all the refuse thrown from the houses. The dwellings here are small, and rather deserving of the name of huts than of houses; their floors are of earth, and are frequently below the level of the streets, while the inhabitants are plunged in misery and vice. It is obvious that such a locality as is above described was particularly well fitted for receiving the poison of cholera in whatever way it might be introduced. The second house that was attacked (*Skjaegger-udgaard*) contained in all nine families, but the disease was exclusively confined to two of these families, thirteen persons in all, who inhabited a single room containing only 1100 cubic feet of air! All these individuals were sunk in the deepest poverty and filth, sleeping on straw and shavings, and several of them were habitual spirit drinkers. The cellar beneath this room had been uninhabited since the preceding winter, and was several feet deep in water. On the 5th of October, the day after the first case of cholera had occurred in the neighbourhood, the owner of the property began to pump out this water, and he continued to do so on the 7th and 8th. The water thus raised from the cellar produced an intolerable stench. When the cellar was emptied, dead cats, mice, and rats, with a portion of the skeleton of a horse, were found on the slimy floor. On the second day of the operation of pumping, the first case of cholera occurred in this house, and in a few days two men, one woman, and four children were attacked, all of whom died. Another family, consisting of a man, his wife, and two children, now entered on the occupancy of the vacated apartment, but were speedily attacked by the disease, which carried off both parents and one of the children. Not one of the other seven families in this tenement was attacked; the malady was confined to this chamber alone; and out of fifteen individuals who inhabited this chamber, ten died. It is difficult, by any theory of an epidemic miasm, aerial or telluric, to explain this remarkable circumstance. Yet for the contagionist a solution of the enigma is provided by the report of C. S. R. Horbye, given at page 62 of this volume. One of the occupants of the above-

named chamber was employed in the house of the first case (the tinman's), on or after the 5th of October, and on the 7th she was seized in her own room with the disease; and from thence it spread to the other occupants of that room. The occupants of the other rooms on the same floor escaped, though, as Mr. Horbye assures us, the sanitary condition of their dwellings and of their persons was not superior to that of the individuals who succumbed to the disease. We confess that this case appears to us strongly to favour the idea of a material contagion confined to one particular apartment. We saw many instances of a similar character during the epidemic in Newcastle in 1853.

In 1850 the third and the sixth week of the epidemic afforded the greatest number of cases.

Professor Holst, after quoting at some length Dr. Sutherland's well-known assertion, that all epidemics would cease to ravage our towns if due sanitary regulations were enforced, proceeds to observe on the improvements effected in the sanitary condition of Christiania between the years 1833 and 1850. Alluding to the mildness of the epidemic in the last-named year, he thinks there is good reason for supposing that the improved sanitary condition of Christiania in 1850, as compared with its state in 1833, enabled the inhabitants to resist more successfully the influence of the epidemic. How rudely must the worthy professor's feelings have been shocked by the ravages of the disease only three years after this assertion was made. Either Professor Holst was in error on the sanitary improvements of Christiania in 1850, or that city had wofully retrograded in this respect by the summer of 1853. Most towns have improved; very few have deteriorated in respect to provisions for cleanliness during late years; but the ravages of disease have not been the less severe. On the contrary, in some places where the population has rapidly augmented they have been more appalling than heretofore; and perhaps it is to the overcrowding rather than to the deficient drainage of these cities that we must ascribe the increase of disease.

The preparations made to meet the epidemic when it was approaching Norway appear to have been ample, and the arrangements for medical attendance on the sick most effective. Our far-seeing northern brethren did not rest quiet till the disease was actually upon them, but made their dispositions beforehand. Daily house-to-house visitation was put in force immediately, and dispensaries and cholera-stations were established; while houses of refuge to receive the families of those who had died of cholera were got ready in the more elevated and healthy parts of the suburbs. The advantage of the prompt removal of the families of those affected, was well illustrated by the results of such a proceeding in the town of Stavanger, on the west coast of Norway. This town contains about ten thousand inhabitants; and when the cholera broke out in any dwelling, the inmates were immediately removed from the room or rooms in which the disease occurred, and these apartments were allowed to stand empty for several weeks, and were thoroughly ventilated and cleansed before any person was allowed to occupy them again. Although the epidemic at Stavanger lasted for not less than fifteen weeks—viz., from the 14th of February, 1850, to the 1st of June, only 90 cases occurred during all this period, and 42 died. We have seen houses shut up after several

persons had died in them from cholera, and, yet, after remaining closed for weeks, the disease reappeared among those who entered upon the occupancy of them. It is a mistake, we think, to close any such apartment; they should be freely left open to allow of the circulation of air through them, and thus the fomites of the disease may be eliminated by dilution with atmospheric air.

The pay of the medical attendants on the sick seems to have been arranged in Christiania on the most liberal scale, and forms a marked contrast with the niggardly remuneration which reluctant guardians and corporations have accorded in this country to those who risked their lives and devoted all their energies and time to combating the pestilence. A sudden invasion of the premonitory diarrhœa took place in the general hospital of the city in the night of the 23rd-24th of October. To this hospital the first cholera patients from Grönland had been brought about ten days before; but no communication between the affected persons and the wards for skin-diseases and surgical cases, wherein this sudden outbreak took place, could be traced. Not less than fifty patients were attacked with diarrhœa and cholerine in the course of that single night. Speedy succour was given, and the malady did not spread further. Only one cholera physician was attacked during the epidemic; but of the nurses, porters, &c., attached to the cholera hospital, not less than eleven were seized with well-developed cholera, and five died. It is probable that these poor people were rendered more susceptible of the disease by the want of proper nutriment, as at first they were permitted to provide their own food, and this was often insufficient, from motives of mistaken economy on their part. The system was immediately changed, and good and nourishing diet provided for the servants of the hospital, and with the most satisfactory results. With the exception of the cholera physician above alluded to, not a single case occurred in those above the rank of artisans. The relative mortality of the sexes was nearly equal—forty-seven males to forty females. The mortality in the cholera hospitals was greater than among those who were treated in their own houses.

The latter part of the week was observed to be marked by the highest mortality. With a view to the more decorous observance of the Sunday, the day for paying wages had been changed from the Saturday to the Wednesday, and the increased consumption of ardent spirits consequent on this arrangement perhaps augmented the severity of the cases that occurred during the latter half of the week. The weather during the prevalence of the epidemic presented no remarkable features, but the "magnetic intensity" was found to be remarkably less than the average of ordinary years.

Professor Faye's report 'On the Origin and Mode of Propagation of Cholera,' is a document of much interest (pp. 81—101). He maintains that the disease was of spontaneous origin, because, in 1850, the introduction of the malady could not be traced. His idea, that the diarrhœa which so constantly precedes cholera is a separate disease, is, we think, a common but a most erroneous doctrine. We regard that diarrhœa as a part of the disease itself, as the direct effect of the cholera poison; and we find, both in this volume and elsewhere, many instances tending to prove that persons labouring under this diarrhœa alone, and without any other

symptom of cholera, may convey to other persons true cholera, which may develope itself in these individuals in the most virulent form. But while Professor Faye writes himself down a non-contagionist, he honestly and boldly confesses that the cholera poison may so infect the air of a room, as to produce the disease in persons who breathe this air. He is not, however, inclined to admit fully the opinion that the fomites of the disease may attach themselves to the clothes of persons in good health, and may thus be conveyed to other individuals at a distance. We certainly have met with instances of this kind; and the facts related by Kierulf before noticed, and the opinions of the reporters of the Royal College of Physicians of London, are favourable to such a doctrine. This indeed seems to be the main point at issue between the strict contagionists and those who, like Professor Faye, admit of contagion in a modified degree; and its bearing on the subject of quarantine must be admitted to be of importance. Professor Faye is a stanch opponent of all quarantine, and he denied that it ever succeeded in keeping out the disease. We hold, on the contrary, that if a quarantine could be absolute and complete, such as has at times been put in practice in small islands, &c., the disease will not be introduced, especially where these islands are separated from the mainland by a certain space of water sufficient to ensure absolute cessation of all intercourse. But in all populous countries, it is perfectly impossible to establish such a quarantine; while at the same time, the misery inflicted on the labouring population of sea-port towns would infallibly depreciate the standard of health among the inhabitants, and predispose them to the malady which, sooner or later, might be introduced among them. The injury to trade and to the national prosperity thus induced would, even supposing that the cholera could thus be effectually barred out, render our labouring classes incapable of resisting the inroads of other epidemic diseases; and fever, diarrhœa, &c., the results of diminished allowance of food, would probably destroy as many as the disorder, thus sought to be excluded, would have carried off: we therefore fully coincide with our Norwegian brethren in urging the removal of all quarantine restrictions, except perhaps in the case of vessels arriving with cholera actually on board, when the patients might be removed to a suitable locality, and debarred for a time from all intercourse with the yet healthy town.

The visitation of cholera in Norway, in 1853, was far more severe than that of 1850. The malady, in its most malignant form, had broke out in Copenhagen, in the month of June of that year; while at that time no other European city but St. Petersburg was suffering from the scourge. Copenhagen had hitherto almost entirely escaped; but the ravages of cholera in that capital were on this occasion most fearful. We have not as yet obtained the detailed reports of the progress of the cholera in Copenhagen and in Denmark, but hope at a future period to lay these before our readers. The appearance of cholera in the Danish capital had awakened the attention of the Norwegian authorities, and every preparation was made to meet the approach of the enemy. On the 25th of July, the first cases appeared in the general hospital and in the town, but for three weeks the disorder made but little progress, so little indeed, that during the first twenty-one days, only 26 cases were reported. By the end of the month of August, the epidemic was at its height, and the daily

mortality was from 90 to 100, while at the same time the malady spread to the adjoining country. On the 14th of December, the town of Christiania was declared free from cholera. The epidemic thus lasted for the unusually long period of sixteen weeks; the number of cases was 2453, or nearly 5 per cent. of the whole population of the town, while the deaths amounted to 1597, or 65 per cent. of those attacked. Though perfectly identical in its symptoms, the epidemic of 1853 presented many deviations from its general course. The districts of the city and of the suburbs which had escaped in 1850, were in many instances now severely visited. In 1833, the first time that Christiania suffered from cholera, the number of cases was 1430; and in 1850, when only a part of the suburbs was infected, the cases were only 153. The suburb of Oslo, which lies high above the river, and is inhabited by a superior class of the population, was not affected in 1850; but in 1853, 96 cases occurred here in a population of about 1000. The filthy streets called the Toiengade and Nordbygade, which lie close to the affected locality of Grönland, escaped altogether in 1850, and suffered but little in the severe epidemic of 1853. In 1850, with the exception of one cholera-physician, all those attacked were of the class of artisans, or else the dregs of the population; while in 1853, the disease raged among the higher classes also; and among these the relative mortality in proportion to those attacked, was actually greater than among the labouring population, being not less than 75 per cent., or 11 per cent. above the average mortality of the whole. Still more singular is it to find that the mortality among the lowest and poorest classes was positively below the general average; for it only amounted to 64 per cent. The deaths in the cholera hospitals were 67 per cent.; the mortality among those treated at their own houses was 62 per cent., or 4 per cent. less than the former. In 1833, the mortality in the cholera hospitals was 6 per cent. in excess of those treated at home; and in 1850, it was not less than 15 per cent. greater, though the disease, as before stated, was then exclusively confined to the most wretched part of the suburbs. Of the attendants on the cholera hospitals, 15 were attacked, and 10 died; but of the 80 medical men engaged in the town, only one was attacked, and he had previously been in a weak state of health, and soon succumbed to the disorder.

The volume before us contains a general report by Professor Conradi, On the Progress of Cholera in Christiania and its Neighbourhood in 1853, followed by very able special reports from each of the five cholera hospitals established in Christiania. The greater part of the rest of the book is occupied by a most excellent general report On Cholera in Norway, by Professor Faye. We deeply regret that the limits to which this notice has already extended, forbid our entering into any detailed analysis of this masterly essay. We do not agree with some of the doctrines here enunciated; but throughout the whole we observe a spirit of candour and of fair inquiry, which impresses us with the truth of the facts at least on which he bases his opinions. In this essay, Professor Faye seems to us to admit more fully the importance of the accumulated facts respecting the propagation of the disease by contagion than he has heretofore done. Indeed, the number of instances here reported of cholera having been first brought into various districts by individuals returning from infected

parishes and towns, and especially from Christiania, where the disease was most severe, is so great, that it is impossible, we think, to deny the fact that this report greatly strengthens the position taken by the contagionists. We have not room here to quote the examples alluded to, but we may briefly state that, out of fifty-eight parishes and towns, the disease is reported as having been introduced by individuals in not less than forty-eight. The malady spread from town to town along the coast, where the densest population and the most constant intercourse is kept up, while its progress into the interior of the country was but of short duration, and the cases few. The disease broke out repeatedly in some towns, apparently from fresh importations from the head-quarters of the disorder in Christiania. Many of those who thus carried the malady back to their homes had not, when in Christiania, visited any cholera patients; it seemed, says Professor Faye, as if a very short stay in the atmosphere of the infected town was sufficient to imbibe the seed of the malady; and such individuals were often first affected with cholera when they had regained their homes. In each of these cases, one or more persons who inhabited the same room—often the wife or other relatives of the traveller—fell victims to the imported malady; while those who waited on the sick occasionally carried the disease still further, and spread it in their own homes. In all this, we own we cannot see anything like an epidemic constitution. Influenza traverses whole country districts, and affects in a single night hundreds living in isolated spots: it does not follow the main routes of communication, nor does it show any tendency to die out in country-places in comparison to its activity in the towns. No country, as we observed at the beginning of this notice, is better adapted for careful observation of isolated cases than Norway; and yet it has been precisely in Bergenstift, on the western coast, where the isolation of the farm-houses is the most complete, that the strongest facts in favour of the contagion of cholera have been collected under the actual observation of intelligent medical practitioners. In addition to those noticed before, we find in the discussions on the mode of propagation of cholera in the meetings of the Medical Society of Christiania, a vast array of most important facts, brought forward by Kierulf, Professor W. Boeck, and others, which to us carry the completest conviction, that here at least, amid the islands of the western coast of Norway, no epidemic influence was at work, but that the disease was spread by contagion, and by that means *alone*. In great towns, when once cholera has obtained a footing, the air respired by so many individuals labouring under the disease becomes, as it were, one vast focus of infection, especially when, as was so markedly the case in Christiania in 1853, a perfect calm continued in the atmosphere for weeks together. Many facts collected of late years have tended to prove that the period of incubation of cholera may possibly be much longer than that of five to eight days, the period which has been generally assigned to it. If quarantine is, therefore, to be maintained at all, it must be persevered in for an almost indefinite period; and the absurdity of any such regulation will be apparent to all.

We cannot leave these volumes without expressing our congratulations to our Norwegian brethren on the completeness and scientific tone of the reports they contain. Small and unpretending as these two volumes are in

comparison to our portentous Blue Books, they yet afford a mine of information on the important subject they treat of, which will tend to raise still higher in the esteem of the medical world the already well-known diligence and scientific attainments of the medical practitioners of Norway.

Edward Charlton.

Review VIII.

1. *Report of the Commission of Inquiry into the Supplies of the British Army in the Crimea, with the Evidence annexed.* Presented to both Houses of Parliament by command of Her Majesty.—1856. Folio, pp. 432.
2. *Appendix to Ditto.* Folio, pp. 196.

When the Report of the Select Committee on the Army before Sebastopol, and that upon the State of the Hospitals of the British Army in the Crimea and Scutari, were published, we gave our readers a brief and general sketch of the movements of the army of the East from the commencement of the war to the close of the disastrous winter of 1854–5, and analysed at some length those portions of the Reports having reference to the causes of that frightful mortality which the army had experienced, to the conduct of the medical officers, and to the general system of management of the department.

Simultaneous with the appointment of a Select Committee by the House of Commons, Government nominated a Commission to proceed to the Crimea, and on the spot to investigate the whole question of the arrangement and management of the commissariat department; the causes of the irregularity and insufficiency of the supplies furnished to the army, with the sources of supply of provisions, forage, and other articles; and to inquire into the alleged delay in unshipping and distributing the clothing and other stores for the use of the troops. The commissioners appointed to conduct this important inquiry were Sir John McNeill and Colonel Tulloch, and it would have been difficult to have found two gentlemen who, from their antecedents, were better qualified for so delicate and arduous an undertaking. Sir John McNeill had commenced his public career as a medical officer in the H.E.I. Company's service, was afterwards employed by them in a diplomatic capacity, and had ultimately been appointed British ambassador to the Court of Persia. Colonel Tulloch, after having served with his regiment in the first Burmese war, had been employed at home, first in the preparation of the 'Army Statistical Reports,' which have been so often noticed in our pages, and in carrying out the sanitary measures founded upon them; and afterwards in organizing and superintending the military pensioners throughout the kingdom—a body amounting to about 15,000 armed men, and probably four times as many not enrolled and armed. It is not our intention to enter upon the question how these Commissioners discharged their duty, to record their subsequent treatment by the Government which appointed them, or to animadvert upon the proceedings before the Board of General Officers at Chelsea. These topics have been of too general public interest to require notice here, and would be out of

place in the pages of a professional journal; but we purpose to inquire how far the Report of the Commissioners corroborates our former conclusions as to the causes of sickness and mortality among the troops, and to notice a few points which were not brought out in the first investigation.

In the course of their inquiry, the commissioners examined the surgeons, or, in their absence, the assistant-surgeons in charge, of every corps serving in the Crimea, as well as the principal medical officers attached to brigades or divisions, and Dr. Hall himself. In addition to the personal examination, written queries were addressed to the surgeon of each corps, the answers to which are contained in the Report. From these documents, together with the returns furnished by the medical officers, Colonel Tulloch drew up a statistical report, showing the extent of sickness and mortality in the various brigades, with the diseases by which these were occasioned; but Lord Panmure decided that such an inquiry was not comprised in the instructions of the commissioners, and directed that it should be omitted, as it would more appropriately come within the scope of those Reports on the health of the army which Colonel Tulloch has been in the habit of preparing for Parliament. The volumes before us, therefore, do not include the interesting details as to the diseases by which the sickness and mortality were caused, nor do they afford any information as to the relative health of the various arms of the service, or of the troops employed on different duties, and occupying different positions during the progress of that protracted siege. We trust that the mass of information on these points, collected with so much trouble by the commissioners, may be hereafter made available to the profession.

The inquiries of the commissioners were confined, in accordance with their instructions, to the state of the army in the Crimea, and had no reference to the previous campaign in Bulgaria. The general mortality of the army by disease during the seven months from the 1st of October, 1854, to the 30th of April, 1855, including also the deaths at Scutari and in the general hospitals, amounted to 35 per cent. of the strength, and fell most heavily upon the infantry corps stationed in the front, which were furthest from their supplies, and had the most severe and never-ceasing duties to perform. The extent of sickness in some of the regiments may be estimated from a fact stated by Staff-Surgeon Young, that a few days after he joined the 63rd regiment (12th January, 1855), "there was not a man for duty, all were sick" (p. 44).

The causes which gave rise to the enormous mortality of 35 per cent. by disease in seven months, may be briefly summed up as improper food, no means of cooking it, insufficient clothing, no adequate shelter from the inclemency of the weather, want of fuel, excessive duty of a most severe and harassing description (including the bringing up of supplies from Balaklava, and digging roots for fuel), want of medicines and medical comforts when sick, and the necessity of treating disease under circumstances which almost precluded the chance of success.

"No effort," says one of the medical officers, "appeared to be made to economise the health of the force. . . . . A little forethought, prudent consideration, and timely preparation, would have obviated the loss of thousands of valuable men during November, December, and January. In the months specified, soldiers were removed wholesale from the scene of usefulness by diseases which may well

be classed under the designation *preventible*, because they would not have occurred to so fearful an extent under favourable modes of living."

The surgeon of the Grenadier Guards attributes the very great difference in the proportion of disease between officers and men chiefly to the circumstance that

"The officers could command a proper diet, had their beds raised from the ground, had changes of clothing, endured less fatigue, and had no manual labour to perform. The officers were in the same description of tents as the men, and had the same exposure to weather and to the influences of climate, but they have been on the whole more healthy in the Crimea than they were in England."

The diet of the men from the period of landing in the Crimea till the beginning of the following February, consisted almost entirely of salt beef or pork, biscuit, green coffee, and rum. The natural consequence was the appearance of scurvy. The men became disgusted with the salt meat, and frequently had no fuel to cook it with; and as there were no means of roasting the green coffee, it was useless, or if attempted to be made use of, injurious. The men were consequently in many instances reduced to live upon the biscuit and rum. On one or two occasions, cargoes of vegetables were brought into Balaklava, but owing to want of transport, the distribution benefited only the corps in its immediate vicinity. Not only was the food of an improper description, but as fuel was not sent up to the front by the commissariat, the men had to procure it for themselves. This could only be done by cutting down timber or brushwood, or by grubbing up roots,—a labour which involved great additional fatigue to the men, already overworked in the trenches and in carrying up stores, while it also deprived them of a portion of their too limited period for repose. The result was, that they frequently would not make the effort to procure it, and either threw away their meat, or in some instances were seen to eat it raw. They are also stated to have suffered greatly from the defective cooking arrangements, as well as the want of fuel and cooking utensils.

"Each man," says the surgeon of the 49th, "cooked his own rations the best way he could, and if a man felt unable to do it, he went without his food."

And the surgeon of the 18th states,—

"One of the assistant-surgeons now present with the regiment tells me that he was five consecutive days without tasting animal food, because he had no fuel with which to cook it. During that time he lived entirely on biscuit and rum." (p. 371.)

The arrangements appear at first to have been equally defective in all the regiments, but by degrees—in some earlier, in others later—men were told off as permanent cooks, who were exempted from all duty in the trenches, a system which seems to have been attended with very beneficial results. The assistant-surgeon in charge of the E field battery observes:

"In the siege-train camp, where many companies were collected together, it was very striking how much healthier those companies were whose commanding officers devoted their first and most strenuous efforts towards building a commodious and well-covered cook-house, from which all the men of the company were supplied."

Might not officers take a valuable hint from this remark as to the importance of paying more attention to the dieting of soldiers, even in the time of peace?

The commissioners have discussed at some length the subject of the soldier's diet, and judiciously observe that—

"The results of this inquiry having clearly demonstrated how indispensable it is to the soldier's efficiency in the field that he shall be supplied with a sufficient quantity of wholesome food, it cannot be desirable that a matter upon which the success of the whole army depends should be left to chance, or to the prudence, activity, and ingenuity of the soldiers individually, even in countries where supplies can be obtained." (p. 47.)

They have accordingly suggested a "standard ration" for troops in the field, and have pointed out certain equivalents which may be substituted for some of the articles whenever economy of transport becomes important. Their observations on this subject are excellent, and derive additional weight from the circumstance that they were submitted to Dr. Christison, who fully concurred in them, and whose detailed observations are published in the appendix to the Report.

During the early part of the winter, the men were very insufficiently clothed, many of them having only one blanket, while their body-clothes were tattered and worn out; and it was not till the month of January that anything like an adequate supply of warm clothing was distributed.

"During the month of December, the severity of the winter had much increased, and the medical officers describe the sufferings of the troops for want of proper bedding, warm covering, and clothing, as very serious." (p. 25.)

The men suffered also very much from want of boots and shoes, those which were sent out from England having been generally found too small, owing to the feet being much swollen from the effects of cold and other causes; the men rarely ventured to take off their boots, lest they should be unable to get them on again. But the men thus badly fed and clothed were still worse housed, their only shelter from the inclemency of the weather being bell-tents, many of which were old, thin, and worn-out. These were not calculated to keep out either the cold or rain, and in wet weather the floor became a perfect mass of mud, in which the men were compelled to sleep without bed or even paillasse. As there was no fuel available, and no means of using it in a tent even if there had been, the only warmth in these wretched shelters must have arisen from the number of men packed in them, amounting to fifteen in each. After the winter was over, wooden huts were erected, and even at that time were of great use in improving the health of the men.

But another very great cause of sickness and mortality was the excessive amount of duty the men were required to perform. The detailed statements on this head, like those on the prevalence and mortality of the different classes of diseases, were deemed by the Minister for War not to come within the scope of the commissioners' instructions, and have consequently not been published, but enough is stated in the evidence of various medical officers to enable us to form some idea of its extent. Thus, the surgeon of the 4th Regiment says:—

"From want of proper transport, the men have had to do the work of mules, with this important difference, however, that the mules have had every night in bed, and the men have not. . . . . I have very frequently known men to be three nights out of bed—one in the trenches, and the two following on guard. I have known them to be four nights out of bed; and on the night of January 25th, we

sent down seventy men—at that time every available man we had—many of whom had been three nights, and some actually four nights, out of bed previously. Moreover, many of the number had, when the party marched off, not returned from Balaklava where they had been sent on fatigue. These men were forced, on their return, to eat their dinner and proceed to the trenches." (p. 323.)

The surgeon of the 17th Regiment, in the end of April, says,—

"Until very lately, the men of the regiment had barely, on an average, one night in bed for one on duty. . . . . There was no amusement, no relaxation, but day and night the same routine of hard work went on."

The assistant-surgeon of the 23rd observes that "the error consisted in attempting to make one man do the duty of three or four," and that, be it remembered, for a continuous period of four or five months, and exposed to the privations already noticed. The surgeon of the 22nd states that—

"From the commencement of the siege, the men had only one night's rest, going again on duty the next evening . . . . and one time, shortly previous to our allies occupying a portion of our trenches, the whole regiment was on duty three nights in succession,—all, of course, not being in the trenches, but on guard on various posts, &c."

Staff-Surgeon Alexander, in charge of the Light Division, has given details relative to the duty performed by the regiments composing it, which show that during part of the winter even the most fortunate corps had not above one night in camp for one on duty, while some had even less; for instance, in the right wing of the 2nd battalion Rifle Brigade, November, 1854, "one company was five nights out of six on duty; three companies seven out of nine." It is unnecessary to adduce any more instances to show the extent of duty during that fearful winter, but it must be borne in mind that in addition to all the regular duties, large fatigue parties were constantly sent down to Balaklava to carry up supplies of food upon their backs, and that, over roads which had knocked up the horse transport of the army! With this history before them, our readers will doubtless concur with Lord West, when he remarks that—

"After their privations from bad food, want of clothing, and excessive work out of all proportion, unheard of in its severity and continuance, the wonder is that a man is left alive to tell the tale."

The preceding remarks bear reference only to the men, we cannot say in health, but who, notwithstanding their physical powers being reduced by privation and exposure, and many of them actually labouring under serious disease, continued, with unparalleled courage, to discharge to the utmost of their strength the arduous duties required of them. But the unfortunate sick who succumbed under the pressure, and were obliged to enter the hospital tents, seem to have fared no better. Owing to the deficiency of marquees, they were for the most part treated in bell tents, of which the commissioners, after a true, though not very flattering description, say:—

"In short, whatever may be the supposed advantages that have led to its adoption as a barrack-tent, it would be difficult to contrive anything much more unfit for the accommodation of the sick." (p. 38.)

Their rations are stated by the surgeon in charge of the 63rd to have been generally only the same as were issued to the other troops; and the surgeon in charge of the 95th observes:—

"At one time the sick were without any other rations than salt meat, biscuit, and raw coffee." (p. 55.)

Even the medicines necessary for the treatment of the cases, and the medical comforts upon which so much of the success depends, were lamentably deficient, and in many instances not to be procured at all. As this is a point which was much contested before the Sebastopol Committee, it may not be amiss to quote the opinions of a few of the regimental surgeons, who are certainly the most unbiassed witnesses upon it. The assistant-surgeon of the 23rd says:—

"The hospital accommodation, medicines, and medical comforts, were miserably deficient, and numbers died in consequence."

The assistant-surgeon of the Coldstream Guards states:—

"As a general rule, I believe I may safely say that of the numerous requisitions made to the divisional stores for supplies of medicine absolutely required by the sick, very, very few indeed have been returned without the medicines applied for being very much curtailed in quantity, or erased altogether, with an accompanying remark, 'None in store.'"

The surgeon of the 4th Regiment says:—

"Until within about the last two months (his answers being dated 29th April), the quantity of medicines and medical comforts has been very limited. They have been doled out in infinitesimal proportions."

The surgeon of the 1st battalion Rifles remarks:—

"I understand from the assistant medical officers, that during the winter great difficulties existed in procuring medicines, the usual answer to indents being, 'None in store;' and that about Christmas the want of opium and Dover's powder was so felt, that for four or five consecutive days parties were despatched to the store at Balaklava, for the purpose of procuring some; but fruitlessly."

And to show that this involved no small amount of labour, he adds:—

"From the state of the roads and the country, it took these people from fourteen to sixteen hours to perform the transit thither and back again." (p. 312.)

The assistant-surgeon in charge of the 3rd Foot Guards states that—

"During the latter part of November, and all December, there was continual difficulty in obtaining the medical comforts required, as well as the medicines. Of the medicines that could not be obtained, were opium and Dover's powder, during the prevalence of diarrhœa and dysentery. Saline medicines, such as carbonate of soda and citric acid, were also deficient during the prevalence of fever. Port wine, arrow-root, and ground rice, were among the medical comforts that could not at that time be obtained when required." (p. 35.)

Deputy Inspector-General Alexander, in medical charge of the Light Division, states in still stronger language the condition of the army in respect to these necessary supplies. In his evidence, taken 15th May, 1855, he says:—

"The supplies of medicines and medical comforts have, generally speaking, not been in sufficient quantities, the requisitions for both being in many instances curtailed; in others, none were to be had; rarely, if ever, were the requisitions complied with in full, until latterly. Three pounds of arrow-root for the whole division were at one time issued, while cholera was raging; at another, of a requisition for sago, arrow-root, essence of beef, and candles, the last only were sent. . . . . As regards medicines, many important ones were at times only obtained in small quantities,—viz., of pulv. opii and calomel, only four ounces of each were got for a sick-list of 636 cases of cholera, dysentery, diarrhœa, fevers, &c., which was about one dose of three grains each of these two medicines to each

patient. Again, one ounce of pulv. opii and four ounces of blue pill were got for the whole division, consisting of nine regiments, besides marines and artillery, the sick-list being 619 of the diseases above mentioned." (p. 144.)

It is unnecessary to multiply these quotations; enough has been stated to show that there was but too good reason for complaint on the subject of want both of medicines and medical comforts, and the painful inference cannot be avoided, that many a gallant soldier fell a victim to disease who might have been saved had the medical officers been properly supplied with the means of treatment.

The question naturally suggests itself, who was to blame for these deficiencies? As Dr. Hall was at the head of the department in the East, the responsibility must rest with him, unless it can be shown that he was neglected by the authorities at home, that supplies could not be procured on the spot, or that he had used every exertion to remedy the deficiencies, but had failed from causes beyond his control. But such does not appear to have been the case. In his evidence before the commissioners, he has made no complaint of neglect at home. He attributes the deficiency, to a certain extent, to the loss of the *Prince;* alleges unnecessary delay in complying with his requisitions at Scutari; incomplete execution of them by the omission of some, and unauthorized reduction in the quantities of other, medicines; and the neglect of the apothecary at Scutari to notify to him the shipment, so that he was not aware of their having been sent. But Dr. Hall appears to have contented himself with writing to the principal medical officer, to admonish the apothecary, instead of taking immediate steps to replace him by an efficient officer. In his evidence he incidentally supplies us with a probable explanation of the irregularities as regards medicines:

"Periodical returns, showing the quantity of medicines received, issued, and remaining in store, are submitted to him by the apothecary at the quarterly periods; but not at any intermediate periods, unless called for. . . . . Has not considered it necessary to call for intimation of any extraordinary demand for any particular medicine." (p. 154.)

To rest content with *quarterly* returns of medicines when in charge of an army situated as that in the Crimea was, the men dying by hundreds, and the numbers sick exceeding those who, though many of them were labouring under disease, continued to do their duty, argues an amount of apathy, or a peculiar constitution of mind, which cannot but excite astonishment. With regard to the possibility of procuring the various articles which were deficient, the assistant-surgeon of the Coldstream Guards states, that when unable to obtain brandy for his cholera cases from the divisional stores, he applied to his commanding officer, who sent to Balaklava and purchased abundance of it; and the surgeon of the 34th states that "Astringents, such as kino, catechu, acetate of lead, were not to be procured, although the medicine bazaar at 'Stamboul' was and is overstocked with these medicines." Surely such sources, open to individual officers, and taken advantage of by them, should not have been overlooked or neglected by the responsible head of the department. Dr. Hall, however, appears to have taken a different view, and in a letter to the chief apothecary, shows an amount of petulance ill becoming an officer of his standing, especially as he well knew the existing difficulties of the regimental medical officers, and the amount of hardship, suffering,

and toil they were then undergoing. On the 27th of November he writes:

"With the creta ppt. you can easily manufacture the hyd. c. cretâ, which is so much run on by some medical officers; and if they cannot obtain the exact article of medicine they require, they say they cannot do justice to their patients." (App. p. 167.)

The commissioners do not leave us in uncertainty as to their opinion of Dr. Hall. They remark,

"Though the correspondence no doubt shows that repeated applications were made by the inspector-general to the reserve depôt at Scutari for medicines and medical comforts, to meet the wants of the hospitals in the Crimea, we feel bound to express our opinion, that at a time when the existence of a great portion of the sick was imperilled by the absence of these supplies, something more than the mere transmission of the usual official demand on the purveyor or the apothecary at Scutari was necessary to relieve the inspector-general of his responsibility; and when he found the inattention to these applications, causing a delay of nearly two months in the arrival of supplies for which the demand was urgent, it appears to us that he ought to have taken some more decided steps to insure attention to his requisitions. A proper officer might have been sent to Scutari with instructions to bring back whatever was most urgently required for the hospitals." (p. 39.)

That the diseases which destroyed perhaps the finest army that ever left the shores of England were *preventible*, is sufficiently proved by the excellent health enjoyed by the troops during the following winter, and by the high state of efficiency of all arms of the service at the close of the campaign. Gratifying as its condition was then proved to be, and satisfactory as have been the results of the nation's exertions, it cannot but be a source of the deepest regret that the lives of thousands of our bravest troops have been sacrificed, and the reputation of our country tarnished, by ignorance and want of foresight at home, and of promptitude and ingenuity on the part of the military authorities and heads of departments of the army in the Crimea. We sincerely trust, that, should it ever again be our lot to engage in a great war, a greater amount of circumspection, prudence, and common sense, may be shown in the selection of the staff to whom the lives of our soldiers are to be entrusted, and by whom the honour of our country must be upheld.

Among the written questions sent to the medical officers was the following:—Have you anything further to state, or any suggestions to offer, with the view of improving the health of the troops? If so, state them. Most of the medical officers have taken advantage of this question to explain their views upon this important point. The suggestions are in many instances given at considerable length, and are well deserving the attention of the authorities. The most important of them may be thus summed up. Reduction in the amount of work; diminution of the weight to be carried by the soldier; an improvement in his clothing, and particularly the adoption of some waterproof covering, and the substitution of flannel for cotton shirts; an amelioration in the quality, nature, and cooking of the rations; the regular issue of vegetables, fresh, preserved, or compressed; the substitution of tea for coffee, and of porter for rum; the establishment of permanent cooks instructed in their duty; the erection of covered cooking places; the adoption of *tentes d'abri* in the field; attention to the ventilation of huts where these are erected; adequate means of ablution; the formation of a sanitary police

in the army; efficient transport for the sick; and a supply of good hospital orderlies and hospital sergeants. The last is a point of great importance, and until the time arrives when these can be furnished in sufficient numbers from the recently-organized medical staff corps, means should be adopted to give the medical officer a voice in their selection, and a greater control over them than at present. The surgeon of the 4th Light Dragoons observes:

"I would recommend that the orderlies be uniformly selected from the well-conducted and able-bodied men of the regiment, and permanently attached to the hospital, subject to removal for misconduct or at the express wish of the surgeon; too frequently when a man becomes unfit for all active duties, or is a dirty, slovenly, badly set-up soldier, he is considered good enough for an hospital servant, and consequently breaks down, either from bodily infirmities or vicious habits, as soon as his services are required most." (p. 418.)

The importance of a good hospital sergeant can be best appreciated by those who have had the misery to be afflicted with a bad one. We cannot afford space to enter fully upon the subject of the recommendations, the importance of which must be fully appreciated by all who have paid attention to the subject of military hygiène.

Since the period of our former notice on the Medical Service of the Army in the East, the Government have recommended to her Majesty to confer the Order of the Bath upon certain of the medical officers of that army.

We regret to learn from our correspondents that the selection which has been made has created a very strong feeling of dissatisfaction. For this, of course, Dr. Smith is responsible, though we presume he has acted upon the recommendations of Dr. Hall. The ground of complaint appears to be, that the order has been conferred rather in consequence of an officer having held a certain position than of his having earned it by his services; and that in some instances it has been bestowed upon officers who left the Crimea before the hardships of that dreadful winter commenced, while others who nobly remained at their posts throughout the whole war were passed over. The selection seems to have been made upon the same principle as among the officers termed "combatant," among whom a very large proportion of the honours were absorbed by the staff, while the hard-working and over-tasked regimental officers were forgotten, or deemed ineligible from their rank. This is much to be regretted, and we trust that on all future occasions the only principle of selection for rewards and honours will be "Palmam qui *meruit*, ferat."

We conclude our remarks with the following warm tribute of praise paid to the indefatigable exertions of the medical officers, by the commissioners:

"The circumstances in which the medical officers were placed, owing to the want of those supplies (of medicines), as well as of every description of diet, accommodation, or comfort, essential to the recovery of the sick, were the most painful that can be conceived. We prefer leaving it to be described in their own words, and shall confine ourselves to the expression of our conviction, that if they were unsuccessful in their treatment to an extent which has seldom occurred in the annals of medical practice, it has not been for want of constant attention and unceasing and energetic efforts on behalf of their patients. The appeals of some of them to those authorities whom they—perhaps erroneously—conceived had the power of assisting them, were most forcible and pathetic; and if all did not make such appeals, it was because they found that when made by others they produced no result." (p. 39.)

Review IX.

The Influence of Tropical Climates on European Constitutions, including Practical Observations on the Nature and Treatment of the Diseases of Europeans on their return from Tropical Climates. By James Ranald Martin, F.R.S. A New Edition.—*London*, 1856.

Many years have now elapsed since the late Dr. James Johnson produced his well-known work on the diseases of tropical climates. After six editions had been exhausted, the demand for it still continued, and Mr. Martin has taken upon himself the responsibility of again bringing it before the public. The present edition, however, is not a mere reprint of the last. Mr. Martin has re-cast and re-written the entire work, retaining a certain portion of Dr. Johnson's observations, but the greater part of it is exclusively his own composition. The present treatise may therefore be considered to a certain extent as a new and original work, and will justify a fuller analysis than we should otherwise feel called upon to afford.

That Mr. Martin has at least had full opportunities of acquiring a practical knowledge of the subject of which he treats we must probably allow, when we learn that he served in various parts of India—in peace and war—amongst natives and Europeans—in hospital and private practice—for two-and-twenty years; that he has turned these opportunities to full account, all those who are acquainted with the sanitary benefits for which the capital of British India is indebted to Mr. Martin's suggestions and exertions, will at once admit. The profession will, doubtless, therefore expect a book full of sound and valuable information, and we think they will not be disappointed.

The first part of the work commences with the consideration of the physiological influences of the climate of the East Indies on European constitutions. The physical climate of Bengal is sketched out, and some interesting information afforded with respect to the geological nature of the soil, which is noticed to be ferruginous in many parts which are notorious for the fatal character of their fevers. The modifying influences of the labours of man are then dwelt on, but our author does not give a very favourable report on the state of agriculture round the metropolis of India; for he states, that with the exception of being able to produce a supply of vegetables for the markets in the cold season, matters are much the same as in the days of Job Charnock, the founder of Calcutta.

An interesting account of the hot and cold seasons, and of their effects on the newly-arrived as well as on the more seasoned European, follows, and we then proceed to the statistics of sickness and mortality in Calcutta.

The subject of statistics is one unfortunately that has been almost entirely neglected by the medical authorities of the Bengal Presidency. It is with great regret that we learn that, owing to this deficiency, no report can be given—

"Of the hospitals of Calcutta, and of the general hospital in particular;—an institution that has existed for more than seventy years, and in which tens of thousands of European soldiers have been treated under three or four different

medical systems; yet no one fact, out of the numerous and important observations made during that long time, is known to any one of us. Its surgeons, and those of the other public institutions,—many of them able and experienced officers,—have, through the neglect of the controlling medical authorities, been rendered, in respect to us and to science, no more than a set of dumb actors in the circle of a routine duty."

Mr. Martin we know has repeatedly remonstrated against this official neglect, but we also know how hard it is to set the official mind right, to stimulate it into wholesome and vigorous action. Owing to the absence of statistical record, it is not easy to give a satisfactory answer to the question as to whether of late years the climate is improved, or the mortality of Europeans diminished. Our author, however, is of opinion that the climate of the actual site of Calcutta is, to a certain extent, more healthy, and that the general mortality amongst Europeans, especially of the better classes, is diminished; but that the chief cause of this will be found in the improved habits of the higher ranks; for, with the troops in garrison, notwithstanding the superior discipline and interior economy of modern times, it would not appear that mortality is much, if at all, lessened; and the same may be said of the nearest military station of former times—Berhampore.

The chapter On the Prevention of Disease contains much that is especially worthy of perusal. Notwithstanding our boasted civilization, we are only beginning to appreciate, as a nation, the higher and nobler offices of the medical profession, in their preventive or prophylactic capacity. In Bengal indeed a systematic plan for requiring from all competent medical officers reports on medical topography, and sanitary statistics of those parts with which they may be best acquainted, has been adopted on the recommendation of Mr. Martin, and his proposals for the sanitary improvement of Calcutta have been carried out under legislative enactments.

In the application of the principles of hygiene to military purposes, the disadvantages of the medical officer are almost endless. In the British armies, whether at home or on foreign service, the rule appears to be to leave even sanitary measures to what are called the proper authorities; the advice of the surgeon is scarcely ever asked, and if volunteered is pretty certain to be disregarded. Mr. Martin mentions, for instance, that once, while serving in one of the most pestilential countries known in India, he made a topographic examination of the localities, and reported the result to his commanding officer, suggesting at the same time what he regarded as the most suitable arrangement for encamping the men against the coming rainy season, when it was well known that a great increase of deadly fever would result. The answer was briefly—"I'll be d—d if I do." The field officer who treated a grave matter of duty in so contemptuous a manner himself paid the penalty of his neglect, for before the year was over he had lost his life.

As M. Thiers remarks, in ordinary histories we see only armies completely formed and ready to enter into action; the effort it has cost to bring the soldier to his post, to feed, to train, and finally to cure him if sick or wounded, are lost sight of. The nation sympathizes deeply with the results of a battle,—the slaughter of a few hundreds by the sword or bullet excites the keenest feelings of regret or of anger; but the tenfold

loss occasioned by mismanagement, by unnecessary exposure to the burning sun or to the heavy dew, by the unhealthy bivouac, or the malarious encampment, passes unnoticed, or is looked on as the necessary consequence of warfare. England, often engaged in great wars, is perhaps of all nations the most slow to learn the real nature of war and of its requirements, and when peace returns, all the experiences of former wars are forgotten in the eager pursuits of commerce.

We may quote here with propriety the remarks with which the author concludes his observations on this interesting subject:

"Finally—it cannot be too often repeated, that on the perfection of the so-called civil establishments—the medical and the commissariat departments—must depend not only the efficiency, but the very existence of our fleets and armies.

"The recent calamitous losses in the Crimea have demonstrated another fact,—viz., that so long as the departments above-named are dependent and subordinate, and under the management and control of officers holding but an inferior rank, consideration, and power, so long will they remain insufficient to their great purposes; but let them once be ruled and directed by responsible officers of a rank and station adequate to command an instant attention to their respective wants—let such officers be largely and immediately responsible—and then, but not till then, will the two most important establishments of our fleets and armies rise so as to be equal to all requirements, whether during peace or in war."

The chapter which follows—the longest in the work—enters minutely into the nature, causes, and treatment of remittent fever,—the subject which the author considers the most important which can engage the attention or excite the interest of the practitioner in tropical regions. He states that on the thorough and complete knowledge of this affection must depend nine-tenths of the usefulness of every medical officer who is called upon to combat the diseases of hot climates, and that he who can treat remittent fever successfully is not likely to be deficient in the skill to manage any other tropical malady. This chapter will no doubt engage the serious attention of our professional brethren in the East, and will amply reward them for the time devoted to its perusal.

The chapter On Fever is followed by an article on the next most important disease of tropical climates and of armies in general, in every region, especially when engaged in active service,—acute dysentery. As William Fergusson states, this is essentially an army disease. "The soldier," he says, "when in the field, may escape fever, but never dysentery, if he lie on the ground." Out of a force of 25,000 men serving in Bengal Proper, between 1823 and 1836, there occurred about 8500 cases of dysentery and diarrhœa. The losses of the French army in Egypt were greater by dysentery than by the plague. During two years and a half only of the time occupied in the Peninsular war, the British army, according to Sir James M'Grigor, lost 4717 men by dysentery, and the last winter campaign in the Crimea has made us only too conversant with the fatal effects of this scourge of armies.

Mr. Martin treats succinctly of this interesting subject in all its known complications, and in its most destructive forms when complicated with scorbutus, as witnessed by himself personally on the expedition to Rangoon during the first Burmese war. He describes it, as well as the forms in which it subsequently appeared in China, and as it has been

seen on the shores of the Black Sea during the war just concluded. The remarks on the predisposing causes, on the best means of preventing this formidable disease, and on the treatment of it when it has actually supervened, well deserve the attention of those surgeons to whom the care of our soldiers and sailors is entrusted.

The next subject which comes under review is the diseases of the liver —diseases which, either as original or as secondary affections, constitute in reality a vast proportion of the sufferings induced by a protracted residence in tropical climates, especially amongst those in active military service. The official returns, indeed, under the special heads of hepatitis, acute or chronic, record but a very inadequate proportion; for when the hepatic affection occurs as a complication or sequela of fevers, dysenteries, or other allied disorders, the cases are classed with the primary disease, the condition of the liver remaining unnoted.

Mr. Martin provides us, however, with a series of statistical tables, illustrating the actual amount of these disorders, of much novelty, interest, and value, for which we must refer the reader to the work itself. We may quote, however, the results of the author's experience with respect to the treatment and probable termination of abscess of the liver:

"When the abscess is small, we may favour the efforts of nature at absorption by care in diet and clothing, gestation in the open air, and change of air, the use of the nitro-muriatic acid, with bitter tonics, &c. When the abscess has happily discharged its contents into the stomach or bowels, or externally, a more nutritious diet will be proper, with the view to support the strength of the patient, and even a little wine may now be allowed. But nothing should be done which may excite the nervous or vascular functions; and mercury in every form is to be carefully avoided, and even the mildest aperients should only be exhibited under necessity.

"Of the instances of recovery within my personal knowledge, by far the greater numbers were amongst such as had discharged the pus by the bowels; and one married lady I remember to have recovered health rapidly, after vomiting the contents of an enormous abscess. I also saw four instances of recovery where the abscess had opened into the lung. Of those in whom the pus was discharged through the external integument, the majority have, within my knowledge, died. But by much the greater proportion of the instances of hepatic abscess end fatally, either before the bursting of the abscess, or within a few hours from its discharge into the peritoneal cavity. And this brings me to the consideration of the propriety of making an opening for the discharge of such abscesses through the external integument. I have often seen this operation performed in India, and I have myself performed it; but in no instance that I remember did the operation appear to me to result in eventual good."

It will be seen that Mr. Martin agrees with Dr. Budd as respects the impropriety, as a general rule, of any surgical interference in these cases.

An article on endemic congestion of the spleen follows naturally upon that on hepatic lesions. The true nature of splenic cachexia is fully entered into, and the fatal nature of the splenic complication, both in malarious fevers and in epidemic dysenteries, carefully pointed out. In the treatment of the chronic enlargement of the spleen, we notice that the combination of tonics and aperients, which forms the general practice in Bengal, coincides very closely with the experience of the natives, who seem to have been long aware of the beneficial results in this complaint

of purgatives, chalybeates, and acids; a very successful native formula, consisting of a mixture of aloes, vinegar, and garlic, with a small proportion of the bazar sulphate of iron, "Kuzees." In Europeans, where the liver is involved in disease along with the spleen, as is so frequently the case, Mr. Martin considers that there is no remedy which, in his experience, can at all be compared in power and efficacy to the persevering use of the nitro-muriatic acid bath, using the combined acids internally at the same time, with bitter infusions, and keeping the bowels freely open.

The chapters On Epidemic Cholera, and On Yellow Fever, in addition to some very interesting historical and topographical notices having reference to the origin and progress of those epidemics, contain a great amount of useful and practical information on the subjects of which they treat. These subjects have, however, been so freely discussed of late in this journal, that we refrain now from any further allusion to them.

Unfortunately, a work relating to the diseases affecting our soldiers and sailors, as well as the civil service, in warm, indeed in any climates, must necessarily include an account of delirium tremens. Some tables furnished by Colonel Tulloch, and given by the author, demonstrate both the frequency and the importance of this affection, the more important as it forms a very frequent complication with many of the acute diseases of hot countries. If we could trust to a report of the statistics of delirium tremens amongst the troops in Canada, during thirty years, by the Inspector-General Henry, we must fear that the habits of our soldiers, in Canada at least, are not improving, the disease becoming more frequent; at least, while in the first fifteen years the ratio of cases to strength was as 1 to 175, in the second fifteen years it was as 1 to 75. We must fear that the same increase extends to other stations, and is still continuing. An interesting table furnishes us at once with proof of the far greater frequency of this affection amongst the troops stationed in our tropical colonies or dependencies, and suggests also some curious and perhaps profitable inquiries as to the causes which give rise to the variations, both in numbers and in mortality, among the different Presidencies. In four years, for instance, in Bengal, the aggregate strength of the European troops being 36,286, the number of deaths was 14, or in the ratio of 1 death to 48 cases admitted. In Bombay, during the same period of time, the aggregate strength being only 18,073, the deaths were 15, or nearly as 1 in 7 of those admitted.

With respect to the prevention of this degrading malady. many practitioners have recommended doses of tartar-emetic or powdered ipecacuanha, to be given at the moment the craving for strong drinks comes on, or to be mixed with the intoxicating liquors, for the purpose of exciting a feeling of disgust. When the person who has made himself a slave to this evil habit of drinking, makes an effort to extricate himself from it, he at first feels himself most miserable, the stomach misses and craves for its usual stimulant, and some substitute must be found. In Canada, according to the experience of Henry, the best medicine under these circumstances consists of a wineglassful of a mixture of the infusion and tincture of gentian, with a little sulphate of magnesia, taken early in the morning, at mid-day, and again in the evening. This would seem

to correspond with the practice which prevails in some of the London prisons, where those who have been in the habit of drinking largely, are allowed, under their compulsory habits of temperance, to drink freely of wormwood tea, which they call for eagerly.

Referring again to military health, we have four very valuable articles —On the Habits of European Soldiers, On the Term of Efficient Service in India, On the Selection and Improvement of Localities for European Troops, and On the Mortality and Physical Management of European Children in Bengal.

In the first of these we find some valuable hints with respect to the improvements which might be adopted in the management of the soldiers. The expediency of finding occupation for the men is strongly advocated. The abuse of ardent spirits, in our various colonies especially, is freely denounced; and another habit—that of using tobacco to excess—incurs justly the author's warmest censure, both as one bad in itself, and as a comparatively new one. He states that he has

"Seen many cases of severe constitutional and cardiac disturbance from its abuse, with perfect recovery of health on the discontinuance of the habit; the digestive functions, those of the heart and nerves, having been seriously affected in the most inveterate smokers. Of the miseries, mental and bodily, which I have witnessed in the persons of young officers from the abuse of cigars, I will only say that they very far exceed those detailed in the 'Confessions of an Opium-Eater.'"

With respect to the selection and improvement of localities for troops, our conduct hitherto has been most reckless and destructive. Appointing as our commanders men, as a rule, ignorant of, and for the most part disinclined even to learn, the most simple rules affecting health; without the official assistance of medical or health officers holding a definite position in the council of military commanders, who might at least give them advice on the subject, the sacrifice of human life to ignorance has been appalling. According to our author, Colonel Tulloch has stated to him, as the results of his investigations in the War Office, that of British soldiers of the Queen's and Company's armies in the East Indies alone, there perished unnecessarily, from 18[illegible] to 1855 inclusive, about one hundred thousand men!

As an example both of the unnecessary destruction of life, and of the benefits to be derived from scientific investigation, we may mention the results as regards the mortality of British troops caused by their removal from the pestilential plains of Jamaica to the healthy mountain regions, first suggested and advocated by Robert Jackson, and finally carried out by the late Lord Metcalfe:

"From 1803 to 1816, and for how long before we do not know, the soldiers perished in Jamaica at the rate of a hundred and thirty per thousand per annum; while by the simple and easily-arranged measure ordered by Sir Charles Metcalfe, the mortality has, since 1842, been reduced to thirty-five per thousand per annum."

The rules and observations on this most important subject laid down by Mr. Martin merit the closest attention.

From the opportunities which our author enjoyed during a long and varied course of service in India, of observing the effects of climate on the moral as well as on the physical nature of Europeans, especially among

the military classes, the conclusion at which he arrives is, that the term of thirty years of actual residence in the East is the very utmost during which persons under ordinary health, and of an average constitution, may be expected to retain their British vigour of thought and action. If this be the case, the evils of an absolute rule of seniority promotion to a scientific corps like the medical department of the army of Bengal must necessarily be great, attended as it is with the detention of the whole administrative staff till long after the period of efficient service in India is passed away.

On the subject of European children in Bengal, Mr. Martin states, that for children of strumous habit, the climate of Bengal and of India generally may be considered favourable. The diseases of childhood run their course very mildly; and, with proper precautions, up to the age of five or six, European children stand a reasonable chance of thriving. At this age, however, they begin to emaciate, to outgrow their strength, to exhibit the necessity for a change of climate. It was at one time imagined that child-bearing was peculiarly fatal to women in Calcutta, that puerperal fever prevailed to a great extent; we learn, however, from the author, that, from whatever cause it may arise, the very contrary holds good at present, parturition being attended with even less risk than in Europe.

"Puerperal fever I never saw in Calcutta; and during sixteen years, in which I was familiar with the state of health among the better classes of Europeans there, I heard of but one death connected with child-bearing, independent of previous disease, and I remember but very few instances of children being born dead."

The alteration may perhaps, to some degree, be accounted for by the fact, that in olden times the management of parturition was left to ignorant native nurses, the public being solemnly warned, in a Calcutta journal of 1780, against the *improper* interference of European physicians!

We have now arrived at the conclusion of the first part, and here we cannot refrain from calling attention to the importance of the subjects of which it treats, to the necessity for a thorough and earnest study of them by all who are called upon to manage the diseases of Europeans in our inter-tropical possessions. The great responsibility which attaches to the medical practitioner in these regions, where the disease is both acute and rapid, where the treatment to be effectual must be both prompt and decisive, is urgently and most properly represented by Mr. Martin, and cannot be too seriously considered.

The second part is no less interesting to the practitioner at home, who is liable to be consulted by the traveller returning with his health more or less shattered, perhaps with organic visceral derangements, or at any rate suffering from the change from a hot to a cold country, obliged as it were to re-acclimatize himself, to alter once more the habits and observances to which his frame had become accustomed. As in the former part of the work, so in this, the investigation of the particular diseases is preceded by an examination of the physiological influences of climate as met with both in the tropics and in temperate regions. Even the children of Europeans in India, who remain there only during the first five or six years of their life, exhibit, after their arrival in this country, an unusual restlessness and mobility of the nervous system, the effect, no doubt, of a

long-continued application of heat. This nervous excitement is associated with a comparative incapacity for study, leading to frequent complaints on the part of the schoolmaster. Fortunately, this condition yields gradually to the effects of time and air, till it finally disappears between the ages of fourteen and eighteen.

Passive congestion, affecting, as the case may be, the cerebral, the thoracic, or the abdominal organs mainly, is the condition most prevalent under European cold, and requires great care on the part of the sufferer, who is only too apt to expose himsef to the damp and cold of our atmosphere. Anæmia, again, is common to fifteen out of twenty of the tropical invalids on their return to Europe. The nervous and muscular systems of the heart and uterus are relaxed and weakened, and hence to a certain degree the diminished power of the forces that circulate the blood, and the intermitting character of the pulse so common to old Indians, as well as the frequency of uterine hæmorrhage in females who have resided long in that country.

These introductory observations are followed by an interesting chapter on the anæmia from which so many tropical invalids suffer. Whatever may have been the nature of the previous acute disease—fever, dysentery, diarrhœa, hepatitis, or cholera—we are pretty sure to find the blood impoverished in quality as well as in quantity. One not uncommon source of this deficiency of blood, is the custom which prevails too frequently in India of putting on an enormous number of leeches, and allowing the oozing to continue. One patient, whose case is mentioned by the author, had, in little more than twelve months, 1200 leeches applied to his side; whilst another had, for weeks in succession, upwards of 150 leeches per week; this last patient calculated, that in six years at least 3000 leeches had been draining him of blood. The general remarks on this subject, with the rules laid down for its treatment, are highly suggestive, and appear to be most judicious.

After some interesting notes on the treatment of invalids suffering from the sequelæ to the fevers of tropical climates, we arrive at the chapter On Chronic Diarrhœa. This affection, which was described by Dr. Baillie as extremely fatal in its character, whether originally contracted as diarrhœa in India, or forming one of the sequelæ of numerous diseases, is seldom simple in its pathological nature—that is, confined to the mucous digestive surface alone.

"On the contrary, a careful exploration of the abdominal regions, coupled with an attentive consideration of all the antecedent and attendant circumstances, will generally show that diarrhœa, in a large proportion of instances, is complicated with, if not mainly dependent on, chronic disorders and diseases of the liver."

The pathology of this affection, the author remarks, has hitherto been ill understood. Whilst chronic dysentery is the result of inflammation of the mucous lining of the large intestine, chronic diarrhœa, as it occurs in Europe, usually presents

"No appreciable inflammation of any portion of the mucous digestive surfaces; indeed, when diarrhœa has existed for any length of time, we find an opposite state of the general system and of the bowels to that of inflammation—an atony of the system, and consequent atony of the digestive functions, and a specially atonic

condition of the discharges from the whole of the secreting organs of the abdomen, including those of the mucous membranes. The brain and nervous centres are likewise in a state of complete anæmia."

In some instances, the diarrhœa is simple in its nature; in too many, it is associated with hepatic or splenic disease.

Chronic dysentery, the next subject treated of, is considered by our author a more manageable disease than chronic diarrhœa, the process of healing an ulcerated bowel in the former being more readily brought about than the restoration of the hepatic function in the latter affection. It must be remembered, however, that many of the cases of severer hepatic complication die off at once in India, and never reach this country. With respect to the treatment, Mr. Martin states that he seldom has recourse to mercury in the chronic dysentery as met with in England. Where there is recent congestion of the liver—from the application of cold, for instance—a few mild doses of mercury, with sudorifics and purgatives, aided by the warm bath, are admissible; when, on the other hand, chronic enlargement of the liver is associated with chronic dysentery, the condition of the system being anæmic, the nitro-muriatic acid bath is preferred to mercurials.

After some very judicious observations on the relaxed condition of the mucous membrane near the anus, with its hæmorrhoidal complications, the author makes some remarks respecting the performance of surgical operations in returned Indians, which it may be well perhaps for the surgeon here to bear in mind. To treat them with justice under these circumstances, he says—

"Requires a careful regard to their peculiar habit—care beforehand and care in the after-treatment being necessary. The nervous system of the returned Indian is excitable, and if but recently from the East, he is often anæmic—conditions of the system which obviously require much attention. I saw a lady but recently arrived in England, in whom the opening of a small encysted tumour nearly proved fatal; and, in the case of a gentleman, the same dangerous symptoms resulted from the removal of a small internal hæmorrhoidal tumour by ligature."

The chapter On Chronic Affections of the Liver is one of much interest. Torpor of the hepatic function under the cold and damp of an European climate would seem, *à priori*, likely to follow on the over-stimulation of the organ under the ardent sun of the tropics:

"To the increased biliary secretion of the hot and rainy seasons in India, followed by proportionate diminution of the hepatic function in each succeeding cold season—alternations extending over many years—a more enduring impairment of function has now succeeded, accompanied generally by great reduction in the reparative powers of the constitution."

This torpor of the liver is usually accompanied by an anæmic state of the whole system, and in some cases is so great, and the case proves so little amenable to treatment, that we are forced to suspect some alteration, more or less extensive, or some permanent injury to the elementary secreting structures of the organ. In the management of the simpler forms of this affection, the author very properly cautions the practitioner against pushing remedies too far, against administering too large and powerful doses—against, in fact, too heroic a line of treatment. The restoration of

the impaired functions must be solicited rather than urged; the debility can only be relieved by gentle and slow degrees.

Where there is reason to conclude that organic changes have taken place in the liver, or that its torpor is associated with induration or enlargement of the spleen, Mr. Martin has recourse at once to the nitro-muriatic bath. It is in this condition, and especially in the chronic enlargement of the liver, that this acid, used as a bath, as well as internally, proves of so much service. First employed in this manner by the late Dr. Helenus Scott, of the Bombay army, for upwards of twenty years, Mr. Martin has persevered in its use, and warmly advocated its efficacy.

The concluding remarks are devoted to the distinction which should be drawn between the free administration of mercury rendered necessary in India, where the symptoms and progress of disease are so acutely violent as to leave but a very few days between recovery and death, and the treatment which should be adopted with respect to the administration of that drug in this country in the returned Indian. In the case of the latter, the patient is usually emaciated, relaxed, enfeebled, and anæmic; the affections are chronic in their nature; their progress naturally slow; in him the free and continued use of mercury, therefore, would be as injurious as in the former case it may be beneficial; and is, accordingly, strongly discountenanced by the author.

To the preceding rapid sketch, we have now but to add the expression of our candid opinion, that Mr. Martin has not only sustained, but materially added to, the reputation which the work in its original form established for him.

Review X.

Cell Therapeutics. By W. Addison, M.D., F.R.S.—*London*, 1856. pp. 84.

The idea that *informs* this work of Dr. Addison's is contained in the following passage from his introduction:

"If cells and nuclei are of such generality and importance as to take rank as agents in normal growth, so that *all* healthy changes of structure and function be referred to them, they can hold no inferior rank, being present in *all* therapeutical reactions against injuries and diseases. The cell doctrine has been gradually advancing its footing in physiology for twenty years; it has also gained a strong hold in pathology; and it seems now required that therapeutical changes in the qualities of the blood should be shown to be either in harmony with it, or capable of a rational explanation without it."

He subsequently follows this idea out to its development in seven chapters—On Reparation, Granulations and Pus, Blood Distempers, Inflammation, The Distinction between Vascular Tissue and Parenchymatous Substances, Organic Disease and the Process of Repair, and Chronic Inflammation. In Chapter VIII. he presents us with conclusions; and the volume closes with a section On Unity of Design and the Physiology of Inflammation.

Dr. Addison is known as an original worker and careful observer; and

his professional brethren will, we are sure, look on any of his labours as just objects of their respect and attention. We proceed to lay before our readers an exposition of the author's views, adding thereto sundry comments of our own.

Taking first his chapter On Cells and Cell Growths, On Granulations and Pus, we find evidence collected to show that cells have special and independent properties—such that one cell may, though in immediate contact with another, form a product visibly unlike that of its neighbour: the chief proofs of this are derived from the vegetable kingdom. He then claims the property of selective absorption for all cells, and especially for the cells of granulations and pus; quoting Kölliker to the effect that cells do not admit every kind of matter indiscriminately, but take up one constituent and reject another. We must say that, to our mind, the evidence for this is exceedingly unsatisfactory, and indeed, utterly overborne by the counter-proofs. Surely, there is no manner of doubt that cells will absorb all manner of things they were never intended to, as well as what they naturally ought. The epithelial cells of the skin or of the intestinal canal absorb mercury brought into contact with them, or arsenic, and a multitude of other matters beside; arsenic applied to the granulating surface of a sore is absorbed, and so it is if laid on the vaginal mucous membrane; the cell-layer on the interior of an ovarian cyst will absorb iodine. We should really think it nearer the truth to say that cells do absorb indiscriminately. Even plant cells, though they will certainly take up from the same soil different proportions of different salts, according to the nature of each individual, yet have no power of refusing solutions of matters that are poisonous to the structure. The view of cells having a special power of selective absorption is invoked to explain the occurrence of abscess and ulceration without hæmorrhage. Dr. Addison thinks the pus cells capable of absorbing the living boundary-tissue in each case, of severing bloodvessels without bleeding, and of making openings in the older vessels for the junction of new ones. How the pus-corpuscles are to do such things we cannot well think. In our view, the process of ulceration is one of gradual decay and liquefaction of a tissue whose normal nutritive action in some way is arrested, it may be by withdrawal of nervous influence, by absence of a due supply of arterial blood, as the result of inflammation or of pure debility. Pus has no essential connexion with ulceration; it represents a quantity of plasma effused in excess, and corpusculating. It belongs more to reparation than to ulceration. In the formation of abscess, we have (take the case of a common phlegmon) effusion of fibrinous fluid and arrest of the normal nutrition of the part; the effused matter suppurates or forms pus, with which the dead (sloughing) tissue is blended; the inflammatory process goes on, more tissue dies and more exudation suppurates, and at length, the most advance being made in that direction where there is least resistance, the boundary is perforated, and the pus escapes. We entirely agree with Mr. Paget, that—

"Inflammation appears to be not only conducive but essential to the spontaneous opening of abscesses; for where it is absent the matter of chronic abscesses will remain, like the contents of any cyst, quiet for weeks, or months, or years; and when, in chronic abscesses or in cysts, inflammation ensues through the whole

thickness of their coverings, it is usually certain that their opening is near at hand."*

The non-occurrence of bleeding is owing to the circumstance that the exudation, the result of the inflammation, compresses and seals up the vessels as it takes place, the blood at the same time coagulating in them. If this exudation be of poor and bad quality, and the blood indisposed to coagulate, the ulceration becomes phagedenic, and bleeding may occur. It is as a sign of the presence and abundance of healthy exudation that pus is termed "laudable." It is not the used or useful part of the exudation, but the excess, that is let go to waste. It is not at all necessary to repair, which, in its best mode, is effected without any of it. The formation of bloodvessels in granulations, continuous with those of the general circulation, is in all probability effected by the process of outgrowth; loops being thrown out by the extension of one capillary channel coalescing with that of another, or in some such way. Again we refer to Mr. Paget's account of this process;† and we would observe that this common simple phenomenon, occurring as it does by a gradual modelling and growth of pre-existing tissue, just puts before us the utter mystery of vital sculpture. It is no cell doctrine or cell physiology that will ever explain to us how it comes to pass that a capillary plexus should extend and ramify through a layer of new exudation. The fact remains as inexplicable as if we had never heard of cells.

In the chapter On Blood Distemper, after certain generalities respecting the action of miasms on the blood, Dr. Addison proposes the hypothesis that the poison of variola acts specially on the glandules of the skin, which being separate, the pustules are discrete; whereas in scarlet fever, "the poison, in virtue of a property of selective absorption, acts most, or primarily, upon the deep cells of the epidermis." He intimates that, in these and like cases, the pus of the pustules, or the desquamating cells of the cutaneous surface, act therapeutically, conveying in themselves the morbid matter out of the system.

"In measles and scarlet fever," he writes, "recovery from the disorder is concomitant with an exfoliation of cell particles from the skin; and that these discharge poisonous matter from the blood seems proved by the contagious properties which it is well known they possess."

We will grant that, in these and such like instances, there may be discerned, dimly shadowed forth, a therapeutic or conservative tendency; but we must say, at the same time, that such lame and impotent efforts are exceedingly unlike the real workings of nature in her methods of cure. Why does a patient get well of variola, or typhoid fever, or any other exanthematous disease? Is it because he has cast out of his body, through the various emunctories, the morbid matter which was vexing it? Surely no: but because *in* his body that morbid matter, that fever-stuff, has undergone such changes that it has ceased to exist. To use the old phrase, the poison has become concocted, *cooked up* (somewhat like railway accounts, and, like them, changed!); and now it is no longer a vexing and mischievous thing, whatever after becomes of it. Contrast with this the case of glanders, and its ever-recurring abscesses. If elimination was the mode of cure, that which nature really must have for relief, then surely

* Surgical Pathology, vol. i. p. 410. † Ibid., p. 215.

we should see more good come of purging, or sweating, or diuresis in fevers; and when they terminated, it would be always by some critical secretion. The pre-Sydenhamite plan of treating variola would prove more successful than that which we now follow. Moreover, if the epidermic-cell therapeutics are of any real moment, anything more than a "*Nebenwerk*," what are we to think of the pulmonary therapeutics? That vast surface which clears off such an amount of poisonous gaseous excreta daily, and from which we know the infectious miasmata do pass off, must surely be far more potent in its eliminating agency than the much less vascular skin. But cell structure has nothing to do with the extrication of the carbonic acid and other gaseous matters from the lungs; mere physical laws effect their transfer from the blood to the atmosphere. Does not this consideration throw cell therapeutics rather into the background? Again, if in variola the pustulation had any really therapeutic intent in the way of discharging the virus, we might surely expect to see the same end attained in a much more natural and less roundabout way by utilizing already-existing cell growth, rather than creating new and abnormal. Having recently examined a variolous pustule with the aid of the microscope, we think we can be sure that the morbid action is not located in the glands of the skin, as Dr. Addison supposes, but occupies a spot of the plain surface. Now, had the object been to eliminate the virus by cell agency, would it not have been far better to have devolved this function on the perspiratory glands than to have raised up a crop of abnormal cells for the purpose? Would not a flow of perspiration have been far better than a flow of pus? The same will apply to the other exanthemata. Such teleological reasoning is quite warranted when we consider and contrast with the so-called cell therapeutics the real conservative or therapeutical actions of nature, which are so well adapted to the purpose they have to serve.

The chapter On Inflammation opens with these words:—"Inflammation is sometimes acute, sometimes chronic. The argument is, that inflammation is new cell growth in the common vascular tissue. The term 'acute' means rapid. Cell growth is a rapid growth." To the argument, thus stated, we enter a decided protest. Inflammation in its most typical instance is quite another thing than new cell growth in the common vascular tissue (by this we believe Dr. Addison means the areolar). The new cell growth is an accident, the mere result of the overflow of plasma, which cannot get through the obstructed vessels, and yet is poured on in abundance through dilated arteries. The proper and true termination of inflammation is gangrene. Arrest of nutritive action is essential to inflammation; and if that arrest be carried somewhat farther, the tissue dies *en masse*, sloughs. Dr. Addison's definition of inflammation would better suit the formation of a fibrous tumour or a cancer. Nor is cell growth within the inflamed vessels an essential part of inflammation; the best authorities are agreed that production of an unusual number of white corpuscles in the blood is no necessary part of the process.

To the inference which he draws from a fatal case of blood-poisoning, the virus being the result of puerperal peritonitis, we make no objection, absolutely. No doubt, the inflammation of the absorbent glands had in some faint degree a beneficial tendency, and was not the cause of death;

yet we hardly think that such an action as this should be considered as mainly therapeutical. To us it appears as an occurrence almost inevitable, by reason of the anatomical arrangement, and not exhibiting any marked conservative effort. A poisoned lymph current irritates the gland through which it passes, and makes it inflame and suppurate;—but what then? Will this adenitis get rid of the poison that has passed into the blood? We trow not. Mr. Simon gives an instance much more to the point, though he does not call it cell therapeutics, when he describes how "a sufferer can commonly identify the material which is producing his diarrhœa as that which had previously been inhaled by him, for it retains its peculiar smell, and imparts it to the increased excretion." Thus, as the result of dissecting a porpoise or a dog—"the evacuations present quite unmistakeably the smell of the one beast's skin, or of the other's blubber." Here it may be fairly supposed the epithelial particles of the Lieberkuhn follicles do really excrete morbid matter from the blood; and such a proceeding as this is worthy of that agency of which it is said, "*Natura nihil agit frustra.*"

In the chapter On the Bloodvessels and Connective Tissue, our author draws just distinctions between them and the various parenchymata; and in the next he endeavours to substantiate the argument, "that when parenchymatous degeneration or organic disease so affects the bloodvessels as to render bleeding imminent, new cell growth in the vascular tissue arises to anticipate or prevent it. Signs of inflammation being combined with the evidences of organic disease." The cases he selects for examples of this therapeutical tendency are "softening of the brain, fatty degeneration of the liver or kidney, and tubercles in the lungs." With regard to the three first, we must confess that we know of no instances in which any such conservative action takes place. Diseased bloodvessels in the brain are the commonest cause of cerebral hæmorrhage, and if softening precedes the hæmorrhage, it surely favours rather than opposes it. Fatty degeneration of the liver is attended with no interstitial cell growth, nor is any similar state of the kidney. In the case of fatty liver, there is marked anæmia of the organ, and no tendency to hæmorrhage; in the enlarged fatty kidney, the refluent pressure of the blood on the Malpighian tufts favours the occurrence of hæmorrhage. We do not see, therefore, how these cases make for Dr. Addison's argument. In pulmonary tuberculosis we have indications of a conservative tendency, provided the morbid process is not intense, in the closing of bloodvessels, the development of layers of false membrane on the pleura covering a superficial vomica, and the formation of a fibroid lining to a quiescent vomica. But in all these instances, surely we must regard the quality of the exudation, and think much more of its consolidating, fibrefying property, than of its cell character. Dr. Addison is writing on cell therapeutics; but we affirm that these are rather instances of fibre therapeutics, and that the more there is of *cell* in them the worse. In the tubercles themselves, and in the pus around them, are plenty of cells, but these are the mischief; as these cease to be produced, and as fibroid production goes on, so does recovery. When a mass of tubercle becomes quiescent and obsolete, it is by the wasting and disappearance of its corpuscles. When Dr. Addison quotes cirrhosis of the liver as an instance in which there "is slow

wasting or decay of the parenchymatous cells of the organ without bleeding, and as gradual a replacement of the lost parenchyma by a growth of fibrous connective tissue which occupies the void," he seems to us to put the cart before the horse. It is the growth of the new fibrous tissue which starves the natural cell parenchyma. The case is an instance of cell-, or rather fibre-, destructiveness, not therapeutism.

The chapter On Chronic Inflammation contains several just and interesting remarks relative to various circumstances which may retard or prevent the process of repair (cell therapeutics); as well as some suggestions why, when no useful purpose can be accomplished, cell growth yet continues. For these we must refer to the work itself. The chapter of Conclusions is very much a *résumé* of what has preceded, and therefore our previous remarks will apply to its contents.

Our own general conclusions respecting the author's views we must state as follows:—*First.* To a certain limited extent, his leading conception is correct, but does not possess, it appears to us, much novelty. Everybody has known now for some good while, that granulations and pus are made up of celloid particles, and every one is familiar with the conception, that the eruptions of the exanthemata are eliminative efforts. We do not see what is gained by applying to such processes the name of cell therapeutics; we may add that, in our own private belief, these actions are not at all really eliminative, any more than a chronic eczema, psoriasis, or pemphigus, which we are very glad to cure without troubling ourselves as to what becomes of the materies morbi. In our view, these and the like are vascular disorders of nerve origin, and have herein a certain affinity with fevers, according to the doctrine respecting the genesis of the latter, maintained so ably by Virchow and Parkes. *Secondly.* The author cuts down his facts too much to suit his idea. He speaks much of the elimination by the skin in variola, measles, scarlet fever; but omits any reference to that by the lungs, or to the alteration, concretion, of the virus in the system. He dwells on the exfoliation of the cuticle about the inflamed joints in gout, as a means of eliminating the materies morbi (giving, however, no proof that such is the case), and says nothing of the tenfold greater evacuation which takes place through the kidneys. He after cites the granulation process of repair, in which there are cells in plenty, but does not even mention the modelling one of Dr. Macartney (much the more preferable), in which cells must be far less numerous. *Thirdly.* He does not seem to consider sufficiently, that if cells may play a therapeutic part in some degree, they may also play a fearfully destructive part. A treatise on cell destructiveness might just as well be written as on cell therapeutics. A cancer or a fibrous tumour is made up of cells, or celloid structure; and the more there is of cells in the cancer, the more intensely malignant is it. A miliary tubercle is full of cells, yet it is a focus of mischief. What is pulpy degeneration of the synovial membrane, with consecutive abscess, but very undesirable and disorganizing cell growth? What is scrofulosis of the absorbent glands but pretty nearly the same thing? What shall we say of ulceration of cartilage, or cystic disease of the testicle, or of the ovary? In fact, the simple truth is just this, that cells occur almost everywhere, and are concerned in almost all operations, healthy or morbid. To dwell, therefore, especially on cell

therapeutics, conveys a false idea, as if cells ministered in some especial and peculiar way to the operations of cure. It does not appear to us that medical science can gain much by such vagueness.'

Before concluding, we would make one remark which this work has suggested to us. The terms, cell doctrine, and cell physiology, which the author uses evidently with full acceptance, appear to us far more pretentious and assuming than our knowledge justifies. That cells enter largely into the composition of many important organs is a demonstrable fact; that nuclei or cells are universally present in developmental periods is also established. These are plain fact, not theories. But what then? what new doctrine on physiology comes of them? Do we understand one whit more how a muscular fibre or a nerve tube lives and acts, because we know that cell nuclei were concerned in their formation? Do we know anything more about the secretion of bile, since the microscope showed us that the liver was made up of cells? Does the abundant presence of cell-growths in the embryo afford us even a gleam of light as to the laws of development? Here comes a liver, and there a heart, and there a kidney, and there certain bloodvessels connected with them; but does the fact (not theory) of cells or nuclei constituting the main part of these organs, explain to us in the very least the enigma of living matter taking such shape and such relations constantly? Both testis and kidney are formed on the same plan, consisting of tubes lined with epithelial cell-growth, whose concernment in the work of secretion we cannot doubt. Yet these cell-growths certainly do not act alike. One *makes* the spermatozoa, and the other does not make, but merely *eliminates* the urinary salts. What gain we, then, from the knowledge of their cell structure? Should we not be about as wise if we merely knew that each organ was made up of tubes? Excepting that in most instances the acts of nutrition and secretion appear to resemble each other more closely than was previously supposed, we do not see that any actual gain has accrued to physiology from what is called the cell doctrine. The liver is made up of cells, and so is the grey matter of the brain; and the physiology of these organs is just where it would be if we were ignorant of their morphology. It is in the things that we cannot see, the secret vital qualities, that they differ.

We have now done; but though we have dissented in many respects from the views advanced in this work, we would not at all wish to speak disrespectfully of it. It is an honest effort in the cause of science; and we hold the belief strongly, that none such is in vain. In the intellectual as in the physical world, stagnation is decay. It is the turning over old soil and old doctrine, bringing new forces and new ideas to bear on old material, opening fresh channels for living matter and living thought, that makes cultivation in both cases effectual and fruitful. For this, and for all his other endeavours in this good cause, we return Dr. Addison our sincere thanks.

Review XI.

1. *On the Results of the Operations of the Gotha Life Assurance Bank for the First Twenty-five Years of its Existence, particularly with respect to the Mortality amongst the Lives Assured.* By G. Hopf, Esq., Manager of the Gotha Life Assurance Bank, and Corresponding Member of the Institute of Actuaries in London.*

2. *On the Medical Selection of Lives for Assurance.* By William Brinton, M.D., Fellow of the Royal College of Physicians; Honorary Fellow of King's College; Physician to the Royal Free Hospital; Lecturer on Physiology and on Forensic Medicine in St. Thomas's Hospital; Physician to the Mutual Life Assurance Society; Examining Physician to the Indian Railways, &c. &c.—*London*, 1856. pp. 58.

The statistics of life assurance companies must be carefully distinguished from the statistics of the population at large, since the aggregate of insurers may be regarded as the most provident as well as the most healthy part of the community. The deductions obtained by an analysis of the numbers presented by life assurance companies can therefore only be put into comparison with the results obtained from similar data; but the data being essentially the same, we may expect, in comparing different countries with one another, to acquire very interesting and valuable conclusions. The value of such comparisons does not, however, only refer to the duration of life or the rate of mortality, but also to social questions and political relations, by which we may account for the greater or less frequency of the withdrawal of policies, of the occurrence of fraudulent declarations, and the like. Thus, Mr. Hopf finds, in comparing the results of the Gotha Company, the oldest German insurance association (established 1827), with the experience of fifteen English companies, that whereas in the former the number of deaths was somewhat greater than of those whose policies lapsed or were withdrawn, in the latter, the number of deaths was 3928, while that of lapsed policies was 11.226. This would appear to bear out Mr. Hopf's observation, that "the German heads of families therefore seem to execute with greater perseverance the resolution they have once taken of being assured;" it is explicable on the ground of the majority of the insurers in Germany having fixed annual incomes, and of the changes of fortune incident to a commercial population not generally affecting them in the same manner as it does the bulk of insurers in Great Britain. The general fact of insurance indicating a vigorous class of the community, is equally borne out in all the countries from which data have been obtained. Thus we find, in Germany and Great Britain, the rate of mortality to be much lower among the insurers than among the population at large. The same has been observed to apply to our trades' friendly societies, as shown by Neison and Finlaison. Both in a scientific and in a moral and practical point of view, these questions are of great importance. The following table is a condensed view of two tables given by Mr. Hopf, in which our readers will find a comparison between the average mortality of different periods of life in the Gotha and other insurance companies and friendly societies, and the mortality of the same periods in mixed populations:

* The Assurance Magazine, July and October, 1855.

Mortality of the Gotha Company compared with that of other Companies and of Mixed Populations.

Ages.	Insurance Companies and Friendly Societies.						Mixed Populations.				
	Gotha Company.	Equitable Society.	Seventeen English offices.	Friendly Societies (Neison).	Friendly Societies (Finlaison).	Prussian Widows' Fund.	England (Farr).	France (Demonferrand).	Belgium (Quetelet).	Hanover (Tellkamp).	Saxony (Leonhardi).
26—30	0·87	0·78	0·81	0·78	0·75	0·70	0·97	0·88	1·50	1·11	1·07
31—35	0·92	0·88	0·89	0·80	0·80	0·85	1·10	0·95	1·58	1·25	0·99
36—40	1·00	1·03	0·99	0·89	0·95	1·09	1·25	0·94	1·72	1·35	1·15
41—45	1·04	1·18	1·13	1·04	[illegible]18	1·35	1·42	1·17	2·16	1·48	1·57
46—50	1·45	1·38	1·43	1·29	1·37	1·74	1·62	1·43	2·43	1·77	2·07
51—55	1·82	1·85	1·91	1·70	1·77	2·24	1·87	1·93	2·47	2·50	2·71
56—60	2·77	2·68	2·65	2·24	2·45	3·11	2·71	2·50	3·15	3·61	3·71
61—65	3·83	3·72	3·79	3·05	3·12	4·60	3·95	4·10	4·24	5·70	5·43
66—70	6·08	5·48	5·55	4·62	4·75	6·57	5·75	5·60	5·83	7·[illegible]1	7·37
71—75	9·01	7·89	[illegible]13	6·85	6·70	10·04	8·32	9·25	8·65	9·34	10·39
76—80	11·35	11·18	11·88	8·84	10·32	13·18	11·94	12·79	12·86	12·74	15·48
81—85	23·94	17·97	17·22	11·97	15·31	19·32	16·90	18·62	18·01	18·50	21·74

It appears from the table that the ratio is particularly favourable to our own population, whether belonging to the class of insurers, members of friendly societies, or the mixed population. The rate of mortality is especially low for the advanced periods of life, and Mr. Hopf, in trying to account for this undeniable feature, admits that we can only come to the conclusion that, in general, mortality is less at the higher ages in England than in Germany.

Another feature which appears to characterize the class of persons who insure their lives, and results from Mr. Hopf's analysis of the Gotha statistics, is the much greater mortality of women at the earlier periods of life; in mixed populations, the reverse holds good. Thus, in the quinquennial periods, 26 to 30, 31 to 35, 36 to 40, the mortality of men is respectively 0·77, 0·88, and 0·98 per cent., while that of women at the same periods of life is 1·66, 1·79, 1·92. After 40, the difference ceases, and at the most advanced periods the females acquire an advantage over males. The Gotha bank do not insure pregnant women, nor have they ever succeeded in determining a case of fraud on the part of a female; and yet, as the author observes, the numbers before us clearly prove that "females understand better than males to gain advantage in the assurance." The following is his explanation of the fact:

"I think we must seek the principal cause of it in the circumstance that women, from the greater bashfulness peculiar to their sex, frequently do not communicate all their bodily infirmities and irregularities to their physicians, much less to others, and feel themselves much less under obligation to give notice to the assurance office of what they consider their own secret respecting the condition of their body."

And again:

"There is no doubt that a greater proportion of females who assure their lives at the younger years, die early. The deviation is too significant and too constant to be considered accidental. We are not able to explain it by any other supposition than by the circumstance that women feel internal hidden infirmities and defects in a higher degree than men, and have a presentiment of approaching danger in consequence of them, which impels them to assure their lives, or that they understand better and more skilfully than men to hide the true state of their health, and to deceive by it even their medical men."

It is, however, to be observed that the greater mortality of females below the age of forty does not apply in England, where the mortality of the two sexes is equal at that period of life. Our own experience would tend to show that this greater mortality among females before the climacteric, in Germany, is due rather to the greater fatality of childbirth, than to the hidden defects adverted to. We throw this out merely as an impression obtained by inspecting numerous returns of foreign agencies, than as a fact, since nothing but the comparison of extended statistics can serve to determine such a question. We should have no difficulty in accounting for the circumstance, if proved to be based in truth, from the much more frequent employment of midwives during labour, in Germany, even among the higher classes, than among ourselves.

We pointed out at the commencement of our remarks on the subject of life insurance, that insurers, as a class, present a much more favourable average duration of life than their uninsured compatriots. This, however, would not be the case, were it not for the surveillance exercised by the police of the insurance companies—their medical officers.

Persons who feel the taint of any disease that may sap their vital power, are even more likely than others to offer to insure their lives, in order to secure a provision for their wives and children. Were they admitted at the ordinary rates, the favourable averages spoken of as peculiar to the insured would soon be reduced below the average of the general population. It can only be by the careful and conscientious appreciation of all the injurious influences to which mankind are subjected, and by a deliberate weighing of the circumstantial as well as the direct evidence bearing upon the health of an individual, that a medical examiner to an insurance company can completely fulfil the duties of his post. He has to guard against nervous anxiety in watching over the interests of his company, quite as much as against a laxity in examining the applicants for the benefits of the institution. The shock to a person in average health on being declined on the ground of some imaginary predisposition, and the injury inflicted upon him by thus refusing him the benefits of assurance, not easily obtained elsewhere when once refused, are matters for the serious consideration of the medical officer of an insurance company. Dr. Brinton, we think, has put together all the points at issue in an extremely fair manner. He shows why it is necessary that the medical man should be made in a measure the guardian of the insurance company, and he gives a succinct outline of the principles by which the medical officer should be guided, as well as practical directions for the best mode of arriving at a satisfactory conclusion. While the limits of a single lecture are necessarily too circumscribed to discuss fully the bearing of various morbid conditions or previous diseases upon the prospects of ultimate longevity, or to enter minutely into the numerous details which are constantly suggesting themselves to medical men who are engaged in this kind of business, we would recommend a perusal of Dr. Brinton's lecture to all young hands who are about to undertake the duties of medical officers to insurance companies, and are not fully aware of the double responsibility which they assume, and probably not entirely acquainted with the points deserving their special consideration, or the manner in which they should be elicited.

PART SECOND.

Bibliographical Record.

ART. I.—*A Report of some Cases of Operation, treated for the most part in Addenbrooke's Hospital, Cambridge, and in the year* 1855. By GEORGE MURRAY HUMPHRY, Esq., Surgeon to the Hospital.—*London*, pp. 67.

A GROUP of such interesting and instructive cases it has rarely been our lot to find within limits so small as those of this unpretending little pamphlet. We congratulate its author on the valuable surgical experiences which have fallen to his share, and still more on the admirable manner in which he has turned them to account.

We will briefly notice the cases recorded in the order which they occupy in the pages before us. The report commences with cases of stricture, in which Mr. Syme's operation of external division was performed. These are three in number, and in each the results appear to have amply justified the course which was pursued. Believing, as we do, that this operation is rightly characterized by Mr. Humphry "as one of the greatest improvements in modern surgery," we hail with satisfaction a new illustration of the fact, that its merits are becoming appreciated by provincial surgeons, not on the ground of authority, but on that of successful experience.

Three cases of division of simple stricture of the rectum follow. The author believes that the successful issue of the proceeding is promoted by division of the sphincter ani at the same time, whether the complication of fistula is present or not.

Mr. Humphry is happy in being able to report two successful cases of amputation at the hip joint. The particulars of each will well repay the reader. An excision of the condyle of the lower jaw, for some morbid condition of that part involving enlargement, in a young woman, aged twenty-one years, is next recorded. Chronic rheumatic arthritis, analogous to that which is familiar to all surgeons as affecting the hip at middle and advancing life, is regarded as the cause. It is to be regretted that no account of any histological examination of the specimen is presented. The next section is headed "Three Cases of Excision of the Knee-Joint." In his remarks upon these operations, the author writes:

> "Although it is a common thing for severe operations upon bones, such as those for necrosis, to be followed by very slight constitutional disturbance, I was nevertheless surprised, and I am sure other persons who witnessed these excisions of the knee-joint were also surprised, at the very slight amount of febrile or other unfavourable symptoms which ensued." (p. 31.)

We do not altogether concur in this feeling, because it must be observed that in no case was there any diseased action in the bones or soft parts entering into or surrounding the joint at the time of the operation. In case No. 2 it does not appear that the cavity of the joint had been affected by any chronic disease, the cause of impaired functions in the limb being an irretrievably damaged patella. These were examples of what has been called an "operation of convenience," their object being not to remove disease, but to promote osseous union between the femur and tibia, in order to render the limb a support to the trunk, instead of an inconvenient appendage, and so they very much resembled the procedure adopted when the two ends of an ununited fracture are sawn off to accomplish the same end. They were not performed for the removal of diseased epiphyses, or while the synovial membrane was in a state of active inflammation. In such cases, and it is in these that excision has been generally applied, the constitutional disturbance has been frequently considerable. The extent to which the object proposed has been attained in Mr. Humphry's cases will be most readily estimated by quotations from his own report. Of the first he writes, after twelve months from the operation had elapsed, "there was pretty firm osseous union between the bones, and every probability of her being able to walk upon the limb." (p. 28.) Of the second, nearly five months after the operation, "the wound had long been soundly healed, and the bones firmly united. He could raise the limb from the bed, and bear some weight upon it; could move about very well with crutches, and there was every probability of his being soon able to walk without them." (p. 30.) Of the third, "the healing of the wound was completed in little more than a month, and in about two months there was firm union between the bones." (p. 31.)

Our space will permit us only to enumerate the remaining cases. One of ovariotomy by the lesser section, and promising to be successful, but cut off on the twelfth day by tetanus; four of tracheotomy; two cases of encysted urinary calculus are given, in one of which the patient submitted to lithotrity thrice, to lithotomy four times by the lateral, and once by the urethro-rectal method, during a period of six years and three quarters; death occurred after the operation last-named, in which a cyst was opened, and the calculus removed from it. In the other case, lithotrity and lithotomy were each performed twice, and a stone successfully removed from its enveloping capsule, a vesical pouch. The details of these cases are extremely interesting, and are well worthy perusal as examples of judicious, persevering, and skilful efforts to deal with one of the most formidable difficulties which engage the powers and tax the resources of the operating surgeon.

ART. II.—*Climate, Weather, and Disease; being a Sketch of the Opinions of the most celebrated Ancient and Modern Writers with regard to the Influence of Climate and Weather in producing Disease.* By ALFRED HAVILAND, M.R.C.S., &c.—*London*, 1855. pp. 144.

THE purpose of this book is the development of a dominant notion which has obtained so large a place in the author's mind as to modify the aspect of every question discussed in its pages. According to him, climate is

the original after which not only man's physical, but his moral, nature is moulded. The climate of Eden was perfect; the sentence of woe pronounced on our first parents was inflicted by means of its deterioration. The herb of the field, which had before been endowed with endless life, became subject to decay; the resulting miasmata "brought death into the world and all our woe." Throughout the world's history, climate has exercised a no less powerful influence on the destinies of man; "in studying climate we study man;" by it "arts, sciences, ethics, religion, war and servitude are regulated" (p. 5); so that, in fact, climatology is synonymous with anthropology. The whole subject may be divided into two parts, ancient and modern. The former is taught in the aphorisms of Hippocrates, "a book filled with well-recorded facts, and some few obvious deductions from them" (p. 10); the latter "in that valuable epitome of knowledge, the Registrar-General's Reports." (p. 11.) The one represents the youth of science, "sportive, imaginative, impressionable;" the other her maturity, "grave, rigid, exact, and unpoetical." (p. 12.)

A chapter is devoted to each of the four constitutions of the seasons, which are described in the treatise on epidemics of Hippocrates; and much loose reasoning is employed in the attempt to show that the statistics of modern times serve to illustrate the truth of the doctrines of the ancient medicine. Of the chapters on the *κατάστασις λοιμώδης*, great part is devoted to the climate of Greece; under which heading many pages are occupied in the discussion of the Hippocratic doctrine regarding the influence of aspect and position on the diseases prevalent in a town or district. According to Mr. Haviland, the ancient Bœotia was like modern Ireland; the country was boggy, and the atmosphere "filled with cold and humid fogs." The ancient Bœotian resembled the modern Irishman, "dull and stupid"! The climate of Arcadia was inclement, and necessitated a constant struggle with the elements; the people, accustomed to a "vagabond and restless life," and "deficient in the first element of patriotism, the love of home," resembled the modern Swiss!

The most useful portion of the work is the series of tables exhibiting the comparative prevalence of certain diseases in London at different periods of the year, projected in curves, the time in weeks being taken for abscissæ, and the variables for ordinates. The diseases selected are phthisis, bronchitis, and pneumonia; small-pox, measles, scarlatina, and typhus; diarrhœa and cholera. In this way the influence of season and temperature on the prevalence of disease is presented to the eye at a glance, and in such a form as to be intelligible with the least possible amount of mental application.

The text is interspersed with numerous passages, not always either aptly or accurately quoted, from classical authors.

ART. III.—*Records of Obstetric Consultation Practice, and a Translation of Busch and Moser on Uterine Hæmorrhage, with Notes and Cases.* By EDWARD COPEMAN, M.D., F.R.C.S., Physician to the Norfolk and Norwich Hospital, &c.—*London,* 1856. pp. 223.

THIS volume, although unpretending in size and character, contains some practical papers which may be perused with advantage. They are re-

spectively upon Puerperal Fever—The Use of the Vectis—The Induction of Premature Labour—Puerperal Convulsions and Craniotomy. The essay on uterine hæmorrhage, although elaborate in its details, possesses, in our opinion, but little novelty or interest, and contains no reference to the latest researches of British obstetricians upon the subject. We shall therefore confine ourselves to a brief notice of the original papers, which in truth embody nearly all that the volume contains of the author's records of obstetric consultation practice.

The employment of turpentine in the treatment of puerperal fever is the principal point dwelt upon in the first paper. Twenty-one cases are reported in which it was given, and of these, fifteen were cured, and six died. The author appeals to these results as being strongly in favour of the medicine; and he is of opinion that there is no other which possesses so much power of controlling the disease. We should be sorry to discourage the use of a remedy so strongly recommended as this is by Dr. Copeman, but we are bound to state that a perusal of his cases has not satisfied us of the justice of the encomiums he has passed upon it. They were, for the most part, of a sporadic character, having been scattered over a period of five years, and were consequently devoid of that danger and intractability which are so common to the epidemic forms of the disease. Occurring, moreover, in private rather than in hospital practice, they cannot be said to have possessed its worst features; whilst many were of so mild a type, that we are persuaded that recovery might have been expected under ordinary treatment. Now, bearing in mind these facts, and that it has been calculated that of any given number of cases of the disease, however occurring, two-thirds, properly treated, may be expected to recover, which very nearly represents Dr. Copeman's success, —that in the hands of our best authorities, it has either signally failed, or occasioned much disappointment,—that of the six cases reported by Dr. Brennan, the original introducer and eulogist of the remedy, one-half died,—that Dr. Copland, whose testimony is so much relied upon by the author, combined with its administration large and repeated doses of calomel, camphor, and opium,—and that of twenty cases in which it was given by Dr. Clarke, not one recovered, we are compelled to dissent from the doctrine that it is possessed of special curative powers in regard to this disease. Not that we have any doubt as to its efficacy in various morbid states of the system which are common to the puerperal period. Thus, for the relief of intestinal irritation, which so frequently imitates or simulates the phenomena of puerperal fever, and also as an external counter-irritant or rubefacient, we know of nothing better; but regarded as an agent possessing specific curative powers, our experience is in accordance with that which we have quoted, and Dr. Copeman's cases do not induce us to alter our opinion respecting it.

The vectis, in the hands of our author, appears to have been a very useful instrument. He remarks that it is available in many instances in which the forceps would be inadmissible, and that as it is equally efficient with the forceps in cases where they may be used, its greater range of applicability (to say nothing of the greater facility with which it can be employed) is an argument in favour of its having its proper share of an accoucheur's attention, if not indeed the preference. Further on the author remarks, that as employed by him, the mechanical power of the

instrument is that of a hook, and that its principal moving power is that of traction. He enters very fully into a description of the instrument he employs, as well as its mode of application; and appends the histories of twenty-four cases, in which he appears to have used it with much success. We cannot here enter into a discussion of the comparative value of this instrument and the forceps, but may observe that we have always been led to consider them as adapted to two different purposes. If mere tractile power is required, we cannot doubt that the general opinion of the profession would be in favour of the forceps; but there is a class of cases occasionally met with in which this instrument is inadmissible, and here the vectis is eminently useful. We speak of malpositions of the head, in which the vertex is either arrested in its descent, or directed backwards instead of forwards. With the aid of the vectis we believe that these and other irregularities might be easily corrected, and thus the progress of the labour very materially assisted. In other words, we conceive that the vectis should be used for the purpose of rectification, and the forceps for extraction. We are well aware, however, that the frequent use of an instrument facilitates its employment, and therefore it is not improbable that, under certain circumstances, the vectis may become a convenient substitute for the forceps.

In inducing premature labour, Dr. Copeman recommends a proceeding not commonly practised, and one which, if it could be relied upon, has some advantages over those generally in use. It consists in passing an œsophagus bougie or tube a distance into the uterus, between its inner wall and the chorion. We need not insist upon the superiority of this proceeding over that of rupturing the membranes, or of its simplicity in comparison with the douche, or the introduction of sponge tents; and two cases appended speak favourably of its efficacy. We do not observe any particular novelty or points requiring notice in the papers on puerperal convulsions and craniotomy. The cases appear to have been treated upon established principles, and are chiefly interesting for their practical details; regarded as such, and as a truthful record of clinical experience, they are well worthy of perusal.

ART. IV.—*The Sanitary Condition of Paddington.* By GRAILY HEWITT, M.D. Lond., Fellow of the Royal Medical and Chirurgical Society, Physician to the Westminster General Dispensary, Lecturer on Comparative Anatomy and Zoology at St. Mary's Hospital Medical School, and one of the Registrars of the Hospital. Author of 'The Pathology of Hooping cough.'—*London*, 1856. pp. 41.

To dislodge an enemy it will not suffice to take a general survey of his position, but it is necessary to attack every point that he occupies. A single battle, crowned with victory, may weaken him, and expose his entrenchments; but to secure permanent success and complete conquest, every advantage must be followed up, until no doubt as to the issue remains. So it is with the great social warfare waging against stinks and impurities of all kinds. We are beginning clearly to know the power of our enemy, and to estimate the extent of his fortifications; but although, to carry on the simile, certain pitched battles must be fought, a guerilla

warfare is equally necessary to expel him from all the dark lanes and filthy courts where he takes refuge.

Dr. Hewitt has followed up the question of sanitary reform as regards the parish of Paddington; and by reference to the Registrar-General's returns and other documents, shows that the laws that apply to towns at large, are equally true of their individual parts; that the health and longevity of the inhabitants stand in a direct ratio with the amount of sewerage, street ventilation, water supplies, and the like, to be found in their district. We conceive that to vestrymen and others interested in the welfare of the parish, Dr. Hewitt's little work must prove very instructive and suggestive.

ART. V.—*An Essay on Intermittent and Bilious Remittent Fevers, with their Pathological Relation to Ozone.* By E. S. GAILLARD, M.D.—*Charleston,* 1856. pp. 59.

THIS Essay has been published partly because, as the author informs us, "by those to whom were intrusted the scales of justice, it was awarded the Annual Premium of the Medical College of the State of South Carolina," "at the period of his graduation." It is the production of a clever student; but though it evinces extended and varied reading, it betrays so much hasty generalization, and so little close and independent observation, that we must differ from those of the author's friends whose "repeated requests" induced him to commit it to the press. Although comparatively a small portion of the treatise, the great feature is undoubtedly the proposition, that the presence or absence of ozone is the efficient element in the arrest or propagation of disease.

It would answer no useful purpose to follow Dr. Gaillard through his mazy arguments; a single quotation may suffice to show both the author's style and his mode of handling his subject. After adverting to the methods employed for generating and testing the presence of ozone, and theorizing on its power of destroying malaria, and, when in excess, of producing pulmonic disorders, he goes on to say:

"We are forced to make a reasonable deduction from the fact of ozone being found by tests, in a 'bottle partly filled with turpentine, and exposed to the action of light and air.' It has long been supposed that the pines of our southern country exerted a chemical effect upon the poison of malaria. This, for a long time vague, conjecture now finds, thus, an interesting corroboration. What more probable than that Nature should, in the magnitude and magnificence of her laboratory, produce the same principle that we, in our miniature efforts, have boasted to originate; or what better calculated to impress us with the happiest conceptions of Omniscience, than that these stupendous agents which produce it should be placed just where this purifying element is most needed and consumed? On reference to chemistry, we find the turpentine to be C^5H^4, and with this, the *now* interesting remark: 'Bottles in which rectified turpentine, not purposely rendered anhydrous, has been preserved, are often studded in the interior with groups of beautiful colourless prismatic crystals, which form *spontaneously;* these crystals contain $C^{20}H^{16}H^6O^6$, or four parts of turpentine with the incidental H^6O^6 *added.*' It needs but a glance to see that we have here the elements of ozone, though not in correct proportions."

It does not appear improbable that the numerous observations that are being made on the presence of ozone at different points and at different periods, may ultimately establish a definite ratio between the presence

of this agent and the existence of certain forms of disease; but we must decline adopting the conclusion at which Dr. Gaillard has arrived,—that we are in a position to found upon our present acquaintance with ozone a theory of the causation of fevers, or of any other class of maladies.

ART. VI.—*Ueber den Verlauf des Typhus unter dem Einflusse einer Methodischen Ventilation.* Von Dr. L. STROMEYER, General-Stabsarzt der Königlich Hannöverschen Armee.—*Hannover*, 1855. pp. 48.

On the Influence exerted by Systematic Ventilation upon the Course of Typhus. By Dr. STROMEYER, Physician-in-Chief to the Hanoverian Army.

ANYTHING coming from the pen of so distinguished an observer and author, and so practised a medico-chirurgeon as Dr. Stromeyer, is certain to command attention among his countrymen. We rejoice, therefore, that he has not thought it beneath his dignity to devote a pamphlet to the subject of ventilation in connexion with the treatment of disease. Ventilation, as a means of prevention and cure, has not yet secured that attention in Germany which it merits; possibly the vagaries that we have been led into in connexion with this subject may, in a measure, have served to deter the Germans from its proper investigation.

Dr. Stromeyer's object is to prove that a uniform current of air playing over a patient is the best curative agent we possess in typhus:

"I hope," he says, "to live long enough to see physicians, when called to a case of typhus, send first to a carpenter, and then to the apothecary, by which means they may obviate the necessity of requiring the services of the former as often as they now do, at the conclusion of their treatment."

Dr. Stromeyer accordingly relies more upon isolation of the patient, and a thorough and constant ventilation of his apartment, than upon any other proceeding. Dr. Stromeyer supports his views by a table of cases treated in his hospital, 77 of which were typhus (under which head he includes typhoid and relapsing fevers), 5 of which proved fatal. He remarks that, with the exception of one, who had an old pulmonary abscess, none would have proved fatal had his method of treatment been adopted earlier, and carried out more rigidly.

Our readers may be inclined to smile at this assertion, but Dr. Stromeyer evidently feels warmly on the matter; and for this reason, as well as on account of the real importance of the object he pursues, we may look over the somewhat dogmatic and peremptory tone that characterizes the pamphlet.

ART. VII.—*A Practical Treatise on the Diseases of the Testis, and of the Spermatic Cord and Scrotum.* By J. B. CURLING, F.R.S., Surgeon to the London Hospital, &c. &c. Second edition, revised and enlarged, with numerous wood engravings.

MR. CURLING's book has already taken its rank among the standard works of the day. The opinions which it contains have been, in various forms, so often before the public, that it will be necessary at present to notice only the improvements which have of late years been made in the treatment of some of the most important diseases of which it treats.

Instead of the ordinary operation for hydrocele, the tunica vaginalis may be emptied by a puncture made with a needle. By acupuncture, anasarca of the scrotum is substituted for a common hydrocele, and the effused fluid is subsequently removed by absorption.

After this operation, it has been observed that "the re-accumulation follows less quickly than after the fluid has been evacuated at once by a trocar, and in many cases does not take place at all." (p. 102.) Iodine injections for the permanent cure of this disease were first employed by Mr. Martin, in India, and have now come into very general use. "The apparatus for iodine injections is simpler and more portable than that which is required for other fluids." (p. 115.) It consists of a medium sized trocar, a half-ounce glass syringe with a metallic nozzle, and a small stop-cock adapted to the canula. The metallic parts are made of palladium. The injection employed by Mr. Curling consists of two scruples of iodine, half a drachm of iodide of potassium, and one ounce of spirits of wine. Two or three drachms of this is injected, and allowed to remain in the sac for five minutes. The fluid is then allowed to flow back through the canula, and about half a drachm is left in the sac.

In the treatment of orchitis, Dr. Fricke, of Hamburgh, first suggested the practice of *compression*. It may be applied both in the acute and chronic forms. The object is to afford support to the dilated vessels. This plan of treatment has lately been very generally adopted, but "when there is much effusion in the vaginal sac, strapping the tumour does not seem to act with much effect."

"Copaiba must not," according to Mr. Curling, "be employed so long as any active disease is going on in the testicle." Our experience in this respect is at variance with his. We have found that copaiba, in combination with sulphuric acid, allays the inflammation of the testicle, as well as that of the urethra, from which the affection of the testicle has commonly its origin. In chronic orchitis, Mr. Curling places his chief reliance upon mercury. "It is desirable to affect the gums slightly, and to keep the patient under the influence of the remedy until all swelling has subsided." (p. 259.) In syphilitic orchitis, mercury is still more evidently required. In tubercular disease of the testicle, on the other hand, mercury is generally prejudicial. (p. 291.) No kind of treatment, either local or general, is of service in cystic disease of the testicle. "The only means that can be adopted is the removal of the tumour." (p. 329.)

Of all the diseases of the spermatic cord, varicocele is by far the most common, and consequently the most important. The improvements that have been made in the treatment of this disease have also been of as great importance as any that have been noticed.

Sir B. Brodie performed the operation for varicocele in St. George's Hospital, by cutting the varicose cluster through its centre. The patient recovered satisfactorily. Sir E. Home cut down upon, and tied the spermatic veins: "Venous inflammation took place, attended with so much constitutional disturbance, that the patient nearly died." (p. 432.)

In order to avoid the risks consequent upon this operation, surgeons now generally pass a needle between the varicose vessels and the vas deferens, and include the former, together with the skin covering them, in a ligature, twisted in a figure of 8 round the extremities of the needle. The

adhesive inflammation only is excited by this proceeding, and the dangers arising from the decomposed and softened contents of the veins (so liable to accompany suppurative inflammation) prevented. The mode of performing this operation *subcutaneously*, however, possesses still greater advantages. This operation is performed in the following manner:

"The vas deferens being separated from the mass of veins, and the latter being pinched up with a fold of the scrotum, a needle set in a handle, with the eye near the point, armed with a double-looped thread, is passed beneath them. When the needle has traversed from one side to the other, the loop is to be drawn out, the needle retracted, and the veins let go, the skin alone being now held up. A second needle, similarly armed, is then to be passed through, over the veins, entering at the same hole by which the first needle was thrust out, and emerging by the same hole at which it entered. The second loop is next to be drawn out, and the needle withdrawn. The bundle of veins is thus included between two double threads, of which one passes over, and the other beneath it. The ends of the thread on each side are then to be passed into the loop of the other, and now, by drawing these ends in opposite directions, the vessels are tied beneath the skin. By this mode of applying the ligatures, the vessels may be either suddenly constricted or be tied gradually by means of a *serre-nœud*." (p. 434.)

This operation has the advantage of not including the skin in the ligature, which we regard as a very material point; for if the skin be included, a certain amount of unnecessary pain is inflicted; and what is of more importance, as soon as the skin begins to ulcerate, the ligature will become relaxed. The circulation through the compressed veins may then probably be restored, and portions of decomposed clot which may have been contained in the vessels, are liable to be carried in the course of the circulation.

In concluding this brief notice of Mr. Curling's work, we heartily recommend it to the notice of every practical surgeon.

ART. VIII.—*On the Nature of Cholera as a Guide to Treatment.* By WILLIAM SEDGWICK.—*London*, 1856. pp. 182.

THE object of this little book is to prove the proximate cause of cholera to consist in functional disorder of the sympathetic system, induced by some impression made upon it through the stomach. Mr. Sedgwick does not appear to claim the proposition as one originating with himself, as he refers to the writings of Dr. Hamilton Bell, Dr. Loder, and others, who have regarded the sympathetic as the part of the organism most immediately involved. Mr. Sedgwick seeks to establish the analogy of the symptoms of cholera collapse with those presented by circumstances accompanying a shock to the sympathetic nervous system, as in the case of a blow applied to the epigastrium, or in that of perforating ulcer of the stomach. He analyses the symptoms accompanying cholera collapse, and dwells more particularly upon the suppression of urine, upon the question of the formation of urea and its vicarious discharge by other organs than the kidneys, the altered condition of the blood, and the relation of the serous discharge to collapse. It is unnecessary to add, that the result of the analysis is entirely favourable to the author's views.

The labour bestowed upon the argument would alone inspire us with respect for the author; and though we are unable to appreciate some of the transcendentalism in which he indulges towards the end of the book,

we regard it as a useful contribution to medical literature. We think that those interested in the important question of the intimate nature of this disease, will find much that is suggestive in our author's ideas; much that is arranged in a new form; and much, probably, that will excite in them a spirit of controversy. For these various reasons, we advise all cholera students to peruse Mr. Sedgwick's volume.

ART. IX.—*A Practical Treatise on Vesicular Hydatids of the Uterus.* By WILLIAM HENRY ASHLEY, M.D., F.R.C.S. E.—*London*, 1856. 8vo. pp. 108.

DR. ASHLEY'S essay embraces the pathological chemistry, etiology, pathogeny, semeiology, prognosis, and treatment of uterine hydatids. It also presents us with a very carefully-compiled history of the opinions of many authors on the subject. The monograph may be read with advantage by those already acquainted with the disease; while to a practitioner who has not yet had any experience in such cases, it will afford considerable assistance.

ART. X.—1. *The Microscope, and its Application to Vegetable Anatomy and Physiology.* By Dr. HERMANN SCHACHT; edited by FREDERICK CURREY, M.A. Second Edition, considerably enlarged.—*London*, 1855. pp. 202.

2. *The Microscope; its History, Construction, and Applications.* By JABEZ HOGG, M.R.C.S., Assistant-Surgeon to the Royal Ophthalmic Hospital, Charing Cross. Second Edition. *London*, 1855. pp. 458.

THE first of these two books professes to be a work on the microscope by a distinguished foreign anatomist, whereas the only part of it which treats of the principal subject is not by Schacht, but by Mr. F. Currey. The enlargements by which this edition is distinguished from its predecessor are the following:—four chapters have been added at the beginning of the book. The first contains Elementary Instruction in Optics: the second is on the English Microscopes; the third on Subsidiary Apparatus; the fourth on the Preservation of Objects. The three last are substituted for chapters on similar subjects which form part of the original work. That the first production of an unknown author should appear as part of a translation, and should thus shelter itself under a distinguished name, is surely objectionable; nor do we think the evil at all removed, even though it may be mitigated, by the fact that it is done with the author's concurrence.

"There is no doubt," says Mr. Currey, "of the superiority of English instruments over those described by Dr. Schacht." This opinion, though opposed to the judgment of anatomists, is shared by our author with other microscopical amateurs; nor is it difficult to understand why this should be the case, if we reflect that while the anatomist regards the microscope merely as an instrument of research, and considers that best which best answers his purpose; the amateur inquires only whether it comes up to a certain standard of perfection, which to the other is of little value. Every one knows that English opticians have arrived at a

marvellous degree of proficiency in the construction of objectives capable of transmitting very divergent pencils of light, or, in other words, of large angular aperture. With such objectives the markings on the shells of certain species of Diatomaceæ can be resolved with a distinctness of definition which no French or German instrument is capable of affording. Much ingenuity has also been displayed in the complicated adaptations by the use of which the excellency of the objectives can alone be made available. Well may the English microscopist value himself on his instrument; well may he leave the coarser investigations to such men as Thuret and Quatrefages, who use the instruments of Nachet; to Tulasne, to Hofmeister, and scores of others great in the mysteries of nature, but uninitiated in those of large angular apertures. To the anatomist, the very perfections of our finest instruments diminish their utility. The eighty guinea microscope of Mr. Ross, to borrow an illustration from Dr. Carpenter, is no more suited to serve his purpose, than is the racer to serve those of the roadster or carriage horse. He has no time to go through a complicated course of adjustment, and even if he had, the result obtained would not be so satisfactory. However carefully the objective of large aperture is used, several disadvantages remain. The outlines of cells and other elements of tissue are so extremely faint as to be difficult to recognise; those points of the object only are distinguishable which are exactly in focus, the parts which are immediately above or below them being wholly invisible. We have not space for the further discussion of this question. After all, there is nothing which can be advanced in relation to it of such force as the indisputable fact, that while no research of any importance in anatomy has been made with the objectives under consideration, results of such immense value have been gained to science by instruments which the English microscopist despises, and considers unworthy even of description.

The second book at the head of this notice, though larger in bulk, is of less pretension than the other, and of more merit. It is intended for the uninitiated, to whom we cordially recommend it as a useful and trustworthy guide. It well deserves the popularity to which it has already attained.

Art. XI.—*A Manual for Midwives and Monthly Nurses.* *Dublin*, 1856. pp. 170.

The propriety of publishing a manual of midwifery for the use of midwives and monthly nurses may appear to many members of the profession rather questionable, so generally is it believed that "a little knowledge," which is all this volume proposes to give, "is a dangerous thing." But we think, on full consideration, it will be admitted that much may be said in its favour. It is obviously to the advantage of the profession, no less than that of the public, that the practice of the accoucheur should be assisted by competent rather than incompetent nurses—that the standard of obstetric knowledge possessed by the several midwives attached to our maternity institutions should be raised; and it is clearly in accordance with the spirit of the times that useful information should be diffused throughout all ranks of the community. Entertaining these views, and persuaded that the profession has nothing to fear from enlightened,

and everything to dread from ignorant, midwives and monthly nurses, we approve of the object of this manual, and are glad to find that it very adequately fulfils the purpose for which it was written. It contains a short description of the pelvis, restricted to an explanation of its more important anatomical and pathological characters—a rather full account of the phenomena of natural labour—good directions for the management of puerperal patients and infants, so far as the nurse's duties are concerned—several useful receipts for articles of diet or drink required in the sick room—and such information respecting the circumstances of difficult, protracted, tedious, and complex labours, as will place the nurse upon her guard, and show when she should seek promptly for the assistance of the accoucheur. The work contains some typographical errors, and a few equivocal idiomatic terms or expressions, which, in a future edition, we would recommend to be corrected and altered. These do not, however, detract from its practical value; but there is one direction given respecting the management of the breasts in the case of persons who do not suckle, which we strongly object to—viz., that "a little milk should be taken away occasionally by the child or by a breast-pump, just enough to relieve the sense of distension." Now, we are satisfied that this is not only an unnecessary, but a mischievous proceeding, and that, for the purpose in view, it is sufficient to restrict the diet, give an occasional cooling aperient, gently rub the breasts with sweet oil when tense, and foment them when painful. This was the practice pursued by William Hunter with almost invariable success at a time when few fashionable people were accustomed to suckle their children; and following the precedent, we have adopted the same practice with equally satisfactory results.

ART. XII.—*Obscure Nervous Diseases Popularly Explained. Being Six Letters, addressed to a Physician, on the many Nervous Affections resulting from Nervous Irritation and other Sources of Reflex Nervous Disturbance.* By J. L. LEVISON.—*London*, 1856. pp. 114.

ALTHOUGH no period of life is equally prone to reflex irritation proceeding from the teeth as early infancy, still, cases occur to every medical man in which, at later dates, anomalous and painful affections are found to result from caries, exostosis, mal-position, or other morbid conditions of these organs. Cases of dyspepsia and facial neuralgia from this cause are familiar to us. Mr. Levison communicates several instances of nervous affections, of a peculiar character, not ordinarily the consequence of dental derangements, which were shown to depend upon that cause. Among the instances given by the author, we may mention a well-marked case of chorea, consisting mainly in convulsive spasms of one side of the neck; another of partial paralysis of one side of the body; another of ptosis; one of sleeplessness, in which cure was obtained by improving the state of the teeth. The subject is one of great interest, and we thank Mr. Levison for the information he affords us. We should be glad to welcome the appearance of a more comprehensive work than the one before us, embracing all the various phases of dental irritation, and addressed, moreover, solely to the medical profession.

PART THIRD.

Original Communications.

Art. I.

On Ulcer of the Stomach. By William Brinton, M.D., Fellow of the Royal College of Physicians, Lecturer on Physiology in St. Thomas's Hospital, Physician to the Royal Free Hospital.

In a former number of this Review (Jan. 1856), I attempted to sum up the information respecting the Pathology of the Gastric Ulcer, that I had deduced from the personal inspection or the records of about a thousand necropsies, in which this lesion had been found to be present.

In the following pages I propose to view the same malady from another aspect; and to analyse, as briefly as possible, the symptoms by which it is usually announced in the living subject. By doing so, I hope to illustrate many of the points alluded to in my former essay, if not to add some interesting details to what is already known respecting the diagnosis of this disease.

However careful and accurate an observer of natural phenomena any one may be, his statements can have little value unless they are accompanied by specific information as to his opportunities and means of observing, their respective extent and delicacy, and the method by which he uses them. For want of such details, we constantly find remarkable discrepancies in the experience of two or more different observers, even where the subjects and instruments of research are such as involve but few and simple conditions of experiment. And when we turn from the simpler physical sciences to one which, like medicine, is not only more complex, but involves mysterious elements of life and disease that our existing knowledge can hardly be said to appreciate, we find such discrepancies far more numerous and prominent. In short, the conditions of experiment are so multiplied, that all strict comparison is impossible.

This proposition will explain the limited use which I have made, in the following pages, of the hundreds of cases of ulcer of the stomach which I have collected from different sources. As records of disease, based on the observations of an almost equal number of independent authorities, they often afford us positive evidence of the most valuable kind; especially when we consider that, in every one of them, the significance of the symptoms noticed during life as evidence of the malady has been certified by the subsequent necropsy. But though their more prominent features offer us what are sometimes very instructive contrasts, yet we cannot jus-

tifiably lay much stress on their minor differences. And any negative evidence we might extract from them would be all but useless. In other words, the absence of all mention of this or that particular circumstance, from any one of these records, constitutes no valid grounds for concluding it to have been really absent from the corresponding case;—an inference to this effect being only allowable where the narrative itself either distinctly expresses the fact, or as distinctly implies it.

But in calling attention to the imperfections of these records, I have no wish to exalt the comparative value of my own clinical researches. On the contrary, I would warn the reader, that equally grave defects are inherent to the observations of any single inquirer.

The successful clinical study of this malady (as of most others) requires the observation and comparison of a great number of cases, at short intervals of time. Such a requirement is best supplied by the out-patient practice of a large hospital. For the class of Hospital patients corresponds, in general, to that portion of the community most obnoxious to the gastric ulcer; which seems to fall with disproportionate severity and frequency on those who suffer from the ills implied by penury in this metropolis—excessive toil, insufficient and unwholesome food, foul air, mental anxiety, and those habits of intemperance which are the effect as well as the cause of such misery. But the disease generally has so chronic and sub acute a character, that the sufferer rarely seeks and obtains admission as an In-patient, except where the hæmorrhage or peritonitis that occurs in its course immediately threatens life.

But Out-patient practice, however conscientiously conducted, offers some features which must not be forgotten when we make it a means of medical research. With opportunities for the most sedulous and minute study of symptoms in a large number of cases, it is often a very treacherous index of their course and termination. The severer examples of disease we may draft into the wards of the Hospital, and thus assure ourselves respecting the progress of the symptoms; and in the event of death, of their exact significance as revealed by the necropsy. But in other instances (which in this particular malady form the majority) the patient proceeds gradually towards recovery from the very beginning of the medical treatment; and in doing so, he often ceases to attend, without any previous intimation, at a variable stage of the cure, such as seems to him to justify his dispensing with further medical advice. The physician is thus left in complete uncertainty as to whether the symptoms disappear or return; and in the latter case, what modifications they offer by comparison with those previously present, and in what way they terminate.

When these facts are taken into consideration, and especially when we remember that, in a large proportion of gastric ulcers, the diagnosis remains somewhat uncertain during a long period of their existence, it becomes evident with what reserve and caution we must receive the most accurate observation of symptoms only. Adding to this uncertainty the bias that even accurate and honest observers often seem to acquire by the study of a special malady, my readers will probably not think me too scrupulous if I regard the majority of cases not confirmed by necropsy as of little service for the study of symptoms, except by careful comparison with others in which this verification has been afforded us.

The statements in the following pages may therefore be regarded as derived chiefly from two sources:—1. The records of some hundreds of cases, affording in many instances little more than an outline of the chief symptoms present, but always verified by careful necropsy. 2. The personal study of about one hundred and fifty cases, affording minute details respecting symptoms, but only verified by necropsy in a small proportion of that number.*

The statements obtained from these two sources exhibit a close agreement with each other. Their discrepancies are, indeed, chiefly negative: in other words, are for the most part explicable by the omissions inevitable in many of these brief records. But whether they are exclusively so, I must for the present decline to decide; and venture to indicate, as the most interesting question for future researches to answer respecting this malady—"How far can the symptoms of gastric ulcer vary from their ordinary formula; or, what anomalies of these symptoms, as carefully observed and recorded during life, are compatible with the presence of this lesion, as proved by an examination after death?"†

In what we may regard as typical cases, the history of the ulcer of the stomach is made up of the following succession of symptoms:—The malady is announced by disturbances of gastric digestion; at first, mere uneasiness and pain in the epigastrium; then nausea and vomiting, or regurgitation, that expel the food previously taken, or a tasteless or acid watery secretion. At this stage of the disease, it is sometimes cut short by the occurrence of perforation, with its sequel of fatal peritonitis. Failing such an accident, the dyspeptic symptoms are next complicated by hæmorrhage from the stomach; sometimes a sudden and dangerous gush, oftener a slow and intermittent drain, of blood. The anæmia produced by this hæmorrhage is generally associated with a cachexia which seems to be essentially independent of it; being chiefly the result of the inanition necessarily implied by frequent vomiting of the food, or by large destruction of the gastric mucous membrane, and consequent impairment of its function. In young females, another symptom is often present, in the form of more or less complete amenorrhœa, which may be associated with either of these two states of anæmia or cachexia; in other words, may be connected with ulceration, with hæmorrhage, or with both. The gradual acquisition of all these symptoms conducts the disease, in a variable period of time, to a climax, from whence we may next briefly trace it towards its termination. Retaining the liabilities to death by perforation, by hæmorrhage, by vomiting, and by exhaustion, which the above organic results of ulceration severally imply, the lesion often ends by one of these modes of dying, or by two or more of them in combination. In other cases, a spontaneous subsidence of these symptoms, in something like the inverse order of their occurrence, announces a recovery; or a similar amendment is only effected by careful medical treatment, such as quite entitles us to dignify it by the term of a cure. In less numerous instances, these symptoms continue with what is (for obvious reasons) rarely more

* The reader who is desirous of referring to examples of these cases themselves, will find several recorded in the Association Journal for the past and present year.

† Further allusions to the same point will be found in a subsequent part of this Essay (p. 179).

than a moderate intensity, during a variable period of life; in the course of which their uniformity is from time to time varied by considerable fluctuations of severity. The remissions which form one extreme of such fluctuations often sometimes merge into intermissions so complete, that we are left in doubt whether the process of ulceration has been merely reduced to a stand-still, or has broken out afresh after the cicatrization of the lesion. In any case, the protraction of these symptoms during many years of life gradually complicates the impairment of nutrition they produce, with that naturally resulting from the approach of old age; mingled with which they then constitute an indirect or conditionating cause of death, the influence of which it seems scarcely possible to estimate with any exactness.

But the symptoms just enumerated vary so remarkably in different cases, that each of them demands a separate study.

Pain, which is usually the first in the order of occurrence, is also the most frequent and characteristic of them all. Indeed, we may doubt whether it is ever absent from the whole progress of any case. For though there seem to have been instances of gastric ulcer, fatal by perforation, in which no pain was complained of prior to the attack that marked this event, still it is obvious that we have no right to presume the absence of so common a symptom as pain in the region of the stomach, merely because a patient has failed to speak of it at the time, or has not referred to it during a brief and agonizing illness. But since, in one or two cases of open ulcer in my own practice, the pain has completely intermitted for several days at a time, shortly before the occurrence of death by exhaustion, we may regard it as just possible that this symptom might be absent during the few days that would sometimes include the whole course of the disease, in cases of rapid perforation.

The character of the pain is peculiar. Rarely or never does the sufferer describe it as* lancinating, stabbing,' or stitching. In the earliest stage of the disease, it is little more than a feeling of weight,† sometimes a tightness, giving the patient an impression as though the food experienced a stoppage in his epigastric region. Retaining these dull and continuous characters, it then gradually becomes intensified into a burning sensation, and at last into a gnawing pain, that produces a kind of sickening depression, which is quite distinct from the nausea often associated with it.

The date of its access is also characteristic. In a vast majority of cases it comes on from two to ten minutes after the ingestion of food, and remains during the one or two hours which correspond to the period of gastric digestion, at the close of which act it gradually subsides and disappears. And when, as is often the case, it is accompanied by vomiting, it almost invariably ceases as soon as this act has emptied the stomach of its contents. In some instances, however, the pain follows deglutition immediately, instead of being preceded by the usual interval of a few minutes. In these cases there is a presumption that the cardiac extremity of the stomach is the site of the lesion: a conjecture which is of course strengthened by embarrassment in the act of swallowing, such as

* The character would often distinguish the pain that attends scirrhus of the stomach.

† These epithets are all derived from the descriptions given by patients themselves.

suggests its close proximity to the œsophagus.* In some instances, the pain imitates that of an ordinary form of dyspepsia, in only coming on half an hour, an hour, or more, after eating.† Lastly, in what are generally either large lesions or protracted cases—often both—the pain loses the above character, becoming continuous during the intervals of the meals, and lasting days or even weeks without any intermission ; or it even occurs chiefly on an empty stomach, and is alleviated by the ingestion of food.

The situation of the pain forms another of its characteristics. The place of its earliest appearance and greatest intensity, and to which it often remains strictly limited, corresponds to the centre of the epigastrium, or to the median line of the belly immediately below the free extremity of the ensiform cartilage. The portion of the epigastric region to which the pain is referred, forms a circular area of rarely more than two inches diameter,—sometimes, indeed, a mere spot of less than half this size.

But there are certain exceptions to the above rule. One of these, which is generally offered in the female, is apparent rather than real, and is due to that change in the situation of the cartilages of the ribs which is effected by the compression of stays, and which materially deepens the epigastric region in the vertical direction. In other instances, the pain is behind the ensiform cartilage instead of below it, or occupies the boundary of the epigastric and umbilical regions, instead of its usual site in the middle of the former. Finally, the pain sometimes lies a little to the right or left of the median line; or extends from a point of greatest intensity here towards either hypochondrium; or, in still rarer instances, is chiefly referred to the latter situation.

In some instances, the pain in the epigastrium is associated with a feeling of violent pulsation or throbbing; in other cases, the same sensation is felt, independently of the paroxysm of pain, which it may even replace. It appears to be analogous to the throbbing of an abscess, and cannot be recognised by any external examination.

The dorsal pain, first described by Cruveilhier, constitutes almost as important a symptom of the gastric ulcer. As far as my experience goes, it generally comes on a few weeks or months later than the epigastric pain, and from this time forth is quite as constant and characteristic, if not as distressing. It is almost always felt as a gnawing pain, which, ranging in its vertical position from the spine of the eighth or ninth dorsal to that of the first or second lumbar vertebra, is usually "interscapular" as well as "rachidian."

Like the epigastric pain, it has a fixed seat, generally remaining at or near the spot of its first appearance during the whole progress of the disease. Like it, also, there are lateral as well as vertical deviations from its ordinary situation. But I do not think I have ever seen these remove it to a greater distance than one or two inches from the median line,—indeed, scarcely ever more than a single inch. Its worst attacks generally alternate—rarely coincide—with those of the epigastric pain.

* One or two instances of this kind have occurred in my own practice. One is also detailed in the Wien Med. Wochenschrift for 1851, No. 51.

† An instance of this kind was lately brought before the Pathological Society by me; and is mentioned in the Lancet for Nov. 24, 1855.

How far the vertical deviation of the epigastric or spinal pain entitles us to conjecture a corresponding situation for the gastric ulcer, I am unable to decide. But one or two cases on record indicate such a connexion:—pain in the umbilical region being, for example, associated with an ulcer of the greater curvature. But with respect to the horizontal deviations of these pains, there is good reason for asserting, that where they are marked, they justify us in inferring a similar situation for the ulcer. The records that I have collected furnish me with about twenty instances of this kind: out of which about fifteen exemplify the concurrence of pain in the left hypochondrium with an ulcer of the cardiac extremity of the stomach; and four or five illustrate the same connexion between the right hypochondrium and the pyloric extremity of the organ. My own practice has also afforded me three or four cases, in which a similar deviation led me to predict the cardiac or pyloric situation of the ulcer during life. But I have found that the very local character of the dorsal pain often makes it a better test than the comparatively more diffuse epigastric pain. While I need scarcely add that the coincidence of the two in respect to this deviation is a far stronger testimony than either, unsupported, can afford; and that even this agreement requires to be confirmed by the presumption which may be derived from the other characters of this symptom.

Among the latter, we may first allude to the effect of pressure in increasing the pain. This is indeed a very important test, being one which, to speak with logical accuracy, converts what may be, for aught we know, a subjective sensitive phenomenon, into an objective one, that constitutes a far more trustworthy indication of local disease. To use the expression which generally suggests itself to the sufferer, there is a soreness as well as a pain: the least pressure in the epigastrium is sometimes unbearable; the patient, if a female, is even content to forego the fancied advantages of her stays, rather than endure the pain the central piece of whalebone in these ingenious aids to disease often produces. In the majority of instances, the soreness is exactly limited to the part of the epigastric region already specified. As it is produced by the more or less direct application of pressure to the diseased structures, it is not to be excited by pressure on the unyielding spine. But, in general, one and the same pressure on the epigastrium will excite both epigastric and spinal pain: sometimes even the latter, chiefly or exclusively.

Of course it is essential to use such a graduated pressure as shall not involve parts more distant than the stomach: a pressure, in short, scarcely exceeding that with which we manipulate the belly in cases of suspected peritonitis or colic. I say this because it would otherwise be possible to make strange errors. Thus I have known cases of mere emphysema and bronchitis, in which deep epigastric pressure caused considerable distress (easily mistakable for soreness), apparently from embarrassing the heart, which had gradually been forced down into the upper part of this region. It is not altogether superfluous to add another caution with respect to the above test. Not only must it be applied with great care and delicacy in the first examination of a supposed case of gastric ulcer, but, as a rule, we can scarcely be too reluctant to repeat it, even to verify an expected amendment. At any rate, its effects are sometimes so injurious, that

it is necessary strictly to prohibit the patient from all manipulation of the epigastric region, as well as from all pressure producible by dress (such as stays in the female) or calling (as is the case with shoemakers).

Whether the pain of a gastric ulcer is always increased by pressure, it seems impossible to decide. There is only one necropsy* on record—and this probably not of a true or spontaneous ulcer—in which it is distinctly specified that pressure was altogether devoid of such an effect. But I have once or twice met with cases which so nearly approached the symptoms of this lesion in all other respects, that I have been obliged to suspect its presence. The varying degrees in which pressure affects the patient in different cases, somewhat confirm such a suspicion, and indicate that even this characteristic of the ulcer is occasionally (though very rarely) absent.

A curious instance lately fell under my notice, in which pressure on the base of the ensiform cartilage relieved the patient from the sense of stoppage, and the dull epigastric pain, which came on soon after eating. Even here, however, there was a circular area, about an inch below the apex of this cartilage, very moderate pressure in which brought on severe epigastric and spinal pain. And after the artificial diminution of the epigastric pain procured by such pressure, the dorsal pain soon became much more violent.

The effect of posture on the pain in different cases is more variable. As a rule, a severe paroxysm is relieved by the recumbent posture, no matter what may be the situation of the ulcer in the stomach. But the varieties of the recumbent posture—or, to speak technically, the *decubitus*—will often have no influence whatever in increasing or diminishing the pain. But in other instances it will afford a valuable confirmation to our diagnosis, and may sometimes even entitle us to conjecture the exact seat of the lesion.

The facts upon which this statement is based are the following:—Of the cases witnessed by myself, in which the symptoms have led me to diagnose an ulcer of the stomach, I have found about two-thirds exhibit a marked influence of posture on the pain. During a paroxysm some were obliged to lie prone, some supine; some on the right side, some on the left, some were even obliged to sit up. I say obliged to do so, including in this phrase equally those cases which were distinctly relieved by the selection of a particular attitude, and those which experienced a great increase of pain by adopting any other. In some of them, however, the painful posture could be borne for a minute or two, until the gradual increase in the severity of the pain forced their abandonment. In like manner, the less painful attitudes had generally been adopted, to the complete exclusion of that habitual decubitus which most persons naturally assume during sleep. The remaining third of my cases offered no peculiarity of decubitus; though in many of them the pain was relieved by rest in the recumbent posture.

The fewer cases in which the effect of posture upon the symptoms during life was compared with the appearances seen after death, have

* Archives Générales de Médecine, 1828, vol. xx. p. 212. A soldier, aged twenty-three, died after twelve days' gastritis. Two ulcers were found.

afforded me more specific information to the same effect.* That is, the first (and probably the larger) group shows a pretty close correspondence between the posture adopted and the site of the ulcer—the prone decubitus being associated with an ulcer of the posterior surface of the stomach; the supine with one of the anterior surface; the decubitus on the right or left side, with a lesion of the cardiac or pyloric end of the organ respectively. But, on the other hand, in some of the very cases where we might have best expected such a connexion, I have found it fail altogether; a large chronic ulcer, exclusively limited to the pyloric pouch, having been associated with no change of an habitual decubitus on the right side; and an ulcer of the posterior surface, or of the small curvature, having been relieved by even the supine variety of the recumbent attitude.†

Such a correspondence in certain cases, and its absence in others, naturally remind one of what may be called objective and subjective sensations in the normal action of nerves, and suggest the analogous distinction of objective and subjective pains in their abnormal states of activity. But such a distinction fades before any philosophical inquiry. Even in the commonest forms of irritation of a nerve, most of the minute mechanical conditions remain unknown to us: and yet until we can specify and determine these, we cannot assert that a given pain is not so far objective, as that it results from a local lesion of the nerve. In like manner, a careful consideration would probably conduct us to the rather startling propositions, that all pain is subjective; that nothing but an elaborate organization at its periphery and centre enables any nerve to give an objective sensation; and that, even then, its objectivity is, strictly speaking, but very partial and imperfect.

The partially subjective character of the pain in gastric ulcer receives a good illustration from the manner in which it is often affected by mental changes. Amongst these we may specially allude to the depressing passions of sudden fear, anxiety, or anger. as frequently bringing on a paroxysm of pain, the severity and duration of which exceed those of the attacks produced by distension of the stomach with food. Here, however, the situation and character of the pain generally remain unchanged.

The effect of movement upon the pain closely corresponds to that of posture. As a rule, all violent bodily exertion is likely to be followed by an attack. While even the moderate exertion implied in walking, sustained so as to produce fatigue, generally brings about the same effect. And there can be little doubt that the relief generally afforded by the recumbent attitude is in great part due to the perfect rest it implies. In some instances which have fallen under my notice, the movements of locomotion have given rise to a peculiar sensation of dragging in the

* Priority in observing this interesting connexion belongs to Dr. Osborne, of Dublin, whose other clinical remarks on the gastric ulcer (Dublin Journal of Medicine, vol. xxvii. p. 361) will well repay the reader

† Even in these cases, however, we may probably distinguish that the decubitus fails to guide our conjectures, rather than absolutely guides them wrong. For the fact that the habitual decubitus remains unaltered, deprives us of all grounds for any inference. Again, in the absence of specific cause to the contrary, we might well expect that the efficacy of the recumbent posture in relieving the pain would be shared by all the varieties of this posture.

right hypochondrium, such as induced me to suspect adhesion of the stomach to the liver. In one of these instances I have been able to verify this conjecture by a necropsy.

With this effect of adhesion I may mention another, that well illustrates the accuracy of an old observation respecting the symptomatology of the liver. In two or three cases, the adhesive inflammation uniting the ulcerated stomach to the surface of the liver, has been accompanied by that pain in the right shoulder which has long been regarded as characteristic of (superficial?) hepatic inflammation.

Lastly, the pain is affected in a special manner by various kinds of food. As already mentioned, its worst access or paroxysm generally has a close (though not exact) correspondence with that period of gastric digestion during which the organ is most distended with food. It is also increased by the ingestion of hard or indigestible substances; and mitigated by a pulpy milk diet. There are also many articles of food which have an irritating effect quite independently of their consistence. Amongst liquids, few are more generally unbearable than ordinary tea and beer. Finally, all hot substances are usually productive of great pain.

But exceptions to all these rules are occasionally seen. The pain is sometimes unconnected with the ingestion of food; sometimes relieved by it. And I have known even brandy, or hot water, to be taken by a patient with this object. While careful inquiry has satisfied me that beer is sometimes (especially in the aged) very well borne by the stomach, and is advantageous to the organism generally.

The *vomiting*, which forms the next symptom of gastric ulcer, is far from exhibiting such specific characters as those just affirmed of the pain which generally precedes it. It usually occurs when the paroxysm of pain has reached its greatest height; forming in this respect the crisis of the attack. Though generally preceded by a few efforts or retchings, it is rarely of a violent character; indeed, the distension of the stomach which prevails at this time, suffices to render it a very easy and painless variety of sickness. Once begun, it rarely ends without completely evacuating the stomach of its alimentary contents:—an act of expulsion which usually affords complete relief to the pain, but sometimes leaves a slight burning sensation, that only disappears after an interval of two or three minutes.

The chief varieties of the vomiting relate to the following details. Firstly, as regards the date of the malady marked by its access. Though generally preceded by the characteristic pain during several weeks, it sometimes comes on much earlier; occasionally almost as soon as the pain itself. In respect to the nature of the substances vomited, these vary chiefly with the precise date of this act. Soon after the ingestion of food they are of course alimentary; at a later period they have an acid character, which is often an intensely sour taste to the patient himself; and still later, are sometimes mixed with bile. Lastly, in those rarer instances in which the act of vomiting comes on quite independently of the ingestion of food:—for example, shortly after rising from a night's sleep—it expels a glairy alkaline fluid, that consists chiefly of the saliva swallowed prior to the attack. In the latter instance, the vomiting (which is often periodic) is frequently connected with habitual drunkenness; especially with the collapse that follows a debauch.

As regards the frequency with which this symptom is present in cases of gastric ulcer, I have no exact and trustworthy estimate to offer. My own experience would induce me to believe that it occurs in the majority of instances, and is rarely or never absent from the whole course of the malady, except in the rapidly perforating ulcer of the young female. The records I have collected afford one example of its absence in an ulcer that remained active during four years. In other cases it has been only represented by slight nausea. In others it has been limited to a single attack, or to the very close of the disease. Such evidence is indeed confirmed by the well-known effect of a strict regimen in alleviating this symptom, which in many cases only comes on after a full meal, and is at once suppressed by reducing the food to a minimum of the most unirritating substances. Among other circumstances that favour the access of vomiting, there is but one which seems to have any very close and constant relation to its frequency and intensity—namely, the size of the ulcer. This is, however, often connected with a long duration of the lesion, and with its intimate adhesion to neighbouring organs: two characters which might independently favour the occurrence of vomiting. The danger of this symptom it is difficult to exaggerate. There can be no doubt that it is the immediate cause of a considerable proportion of the deaths from gastric ulcer; indeed, my own experience entitles me to predict that more exact information will hereafter exhibit the mortality thus produced as one which far exceeds that brought about by hæmorrhage, and approaches the large per-centage due to perforation.*

It is not difficult to explain the dangerous character of this vomiting. By expelling the greater part of the food shortly after its reception into the stomach, it starves the patient with a rapidity that will be determined chiefly by the quickness of its access, and the completeness with which it empties the organ. And in addition to the effects of inanition, it adds the fatigue implied by such violent and abnormal action of the nervous and muscular systems.

The next symptom of the gastric ulcer is *hæmorrhage.*† Since the process of ulceration itself implies a solution of continuity‡ in the coats of some of the vessels of the stomach, nothing short of a simultaneous

* Compare the author's Essay in the January number of this Review, p. 180.

† I have purposely avoided the use of the word *hæmatemesis* in speaking of hæmorrhage as a symptom of gastric ulcer One chief reason for my doing so has been the fact (till now too little noticed in the history of this malady), that blood effused into the stomach often escapes being vomited thence, and can only be detected in the fæces. And as it would seem pedantic to burthen our professional vocabulary with the addition of some such word as *hæmatokopræsis* to express the latter (and even more frequent) of these two results of hæmorrhage into the stomach, I prefer to lay the chief stress on the pathological occurrence, rather than on its obvious symptomatic consequences. There are still more valid objections to the use of the term *melæna*, which, though bequeathed us by Hippocrates, implies both vomiting and purging, and expressly connotes a peculiarity of the matters thus expelled from the alimentary canal—namely, their black colour—which is neither constant nor essential.

‡ With respect to the occurrence of "hæmorrhage by exhalation," through the coats of the vessels, apart from any solution of their continuity, little need be said. "We now know that this doctrine is incorrect, that the walls of even the finest capillaries have no pores of appreciable magnitude, such as would be necessary for the transit of blood-corpuscles; and hence that the extravasation of these structures is a proof that some bloodvessel has been ruptured. That amongst the myriads of these minute tubes present we often fail to detect the exact seat of the lesion, need of course little surprise us."—[Art., Stomach and Intestine, by the author, in the Cyclopædia of Anatomy.]

obliteration of these tubes can prevent some effusion of their contents. And hence it is not very surprising to find that the myriads of such tubes concerned in every lesion are rarely occluded with that quickness, precision, and universality, which would be requisite to suppress all hæmorrhage from their interior.

How far the hæmorrhage which occurs in ulcer of the stomach may be attributed to mere congestion, it is impossible to determine. But from the analogy of this lesion to ulcers seated elsewhere, we may fairly presume that the same degree of congestion which generally attends ulceration might constitute an efficient immediate cause of the bleeding. Still, as the hæmorrhage almost invariably occurs soon after a meal, and is often distinctly traceable to the ingestion of an unusual quantity of food, we are left in doubt whether the influence of this inflammatory congestion is not far surpassed by that afflux of blood which attends the act of gastric digestion, as well as by the mechanical disturbance which distension of the stomach would necessarily inflict on the diseased vessels that occupy the ulcer itself.

In my former essay I have alluded to the varieties of hæmorrhage in respect to their source, and have classified them as coming from the minute vessels of the gastric coats; from the chief branches or trunks of the arteries which run in the sub-serous areolar tissue external to the stomach, before being distributed to these coats; and lastly, from the vessels that occupy the parenchyma of the adjacent liver, spleen, and pancreas, and become involved in the ulceration after the stomach has contracted adhesions to one or other of these organs. I have also noticed the relation of these varieties to the depth—and therefore, other things being equal, to the date and duration—of the ulcer, as well as to the amount and appearances of the blood effused.

There are no data for determining the frequency of those scanty hæmorrhages which are poured out, in the earliest stages of the ulcerative process, from the minute vessels of the mucous membrane, and its sub-mucous areolar tissue. But it is certain that they occur in a large majority of cases. And it is probable that they are present in numberless instances, in which no symptom reveals them. For since a small quantity of blood does not excite vomiting, it depends entirely upon a casual coincidence of these two symptoms—hæmorrhage and vomiting—whether the former is revealed by the latter. And unless the attention of the patient be particularly directed to the examination of his stools, a moderate quantity of blood may also leave the intestinal canal by this natural channel, without ever being detected.

In all cases of this kind, the blood undergoes the ordinary changes which attend its exposure to the action of the fluids of the stomach and intestine. The moderate quantity of blood generally poured out not only becomes mingled with the various ingesta and secretions which may chance to be present, but gradually undergoes a kind of digestive process, that has the effect of greatly modifying its colour and consistence. Wherever the extravasated blood has been sufficiently exposed to this action, it will be found to have acquired a dark, grumous, or even black colour, and a peculiar tarry or almost pultaceous consistence. A small quantity of blood thus altered by digestion sometimes even simulates the colour and appearance of inspissated bile.

Hence the following precautions are often necessary in respect to this important symptom of gastric ulcer. We must never presume its absence because the patient has failed to notice it. Our inquiries must be directed equally to the matters vomited and to the stools. As regards the former, we must question the patient, not only as to what he may have recognised as blood, but as to the character of all the substances he has vomited. And the matters habitually rejected from the stomach should be submitted to a strict and repeated microscopic examination; care being taken to select such specimens as are either free from all admixture of food, or at any rate from animal food containing blood-corpuscles. Precautions of this kind will often show that a comparatively clear fluid deposits a sediment containing blood-corpuscles in considerable quantity; and perhaps ranges in other specimens from the same patient, through a brownish ropy mucus, to a grumous fluid having the ordinary "coffee-grounds" appearance of blood thus altered by digestion. A similar examination will sometimes be useful in the case of the blackened fæcal evacuations to which gastric hæmorrhage gives rise. Dilution with water will generally distinguish inspissated bile. But if not, the microscope will at once set the question at rest. The ingestion of the salts of iron is a source of error that may of course be easily detected by inquiry; though I have known the inky vomiting which has accidentally followed the administration of this drug immediately after tea, excite considerable alarm in the mind of a patient and his medical attendant.

The proportionate frequency of those larger hæmorrhages which are due to the vessels external to the stomach becoming involved in the ulceration, is just as uncertain. But from my own experience I should conjecture that they occur in not more than one-third of the gastric ulcers which come before us in ordinary practice. And I have elsewhere adduced reasons for supposing that they are fatal in from three to five per cent. of the whole number of these lesions which the most sedulous examination can detect in the dead body.

The symptoms of such hæmorrhages illustrate and confirm the proposition implied above—namely, that the blood poured out from the ulcer into the stomach scarcely exerts any specific action as an emetic or a purgative, but seems to excite vomiting or diarrhœa chiefly by its quantity; in other words, by the mechanical stimulus which its distension of these segments of the intestinal tube implies. Soon after a meal, the patient begins to experience an unusual fulness and weight in the region of the stomach; attended (sometimes even preceded) by feelings of syncope. Nausea rapidly supervenes, and ends in the vomiting of a large quantity of blood; which may either be partially coagulated, or, if rapidly effused and rejected, may retain a colour and fluidity that testify to the arterial character of its source. In other cases (and I am disposed to conjecture, chiefly in those rarer instances where the hæmorrhage, besides being less considerable in quantity, occurs independently of the meal time) the blood is effused in considerable quantity without exciting any vomiting whatever; and is passed at once, through the pylorus, into the intestine, which it leaves more or less rapidly with the stools. Lastly, in very exceptional cases, the rapidity of the hæmorrhage is so great that it distends the stomach, and more or less the intestine, with a single gush; and the

patient faints and dies before any expulsive act can take place, or diminish the enormous clot which the necropsy reveals as the cause of his sudden decease.

The state of the bowels in this malady seems devoid of all connexion with any special features of the lesion. Constipation is, however, the rule in the great majority of cases. And diarrhœa is so much the exception, that we may doubt whether its frequency is much greater than might be expected, supposing it quite independent of the ulcer. But, as already intimated, copious hæmorrhage from the lesion generally gives rise to looseness of the bowels, though without producing any modification of the ordinary epigastric pain. A significant contrast to these facts is afforded by ulcers situated in the first portion of the duodenum (or the immediate neighbourhood of the stomach), which give rise to diarrhœa with much greater frequency than the gastric ulcer. There can be little doubt that this difference is due to that simple law of the peristalsis of the alimentary canal, which connects the movements of the most distant parts of the intestine, while it confers a comparative isolation on those of the stomach.

Amenorrhœa is present as a symptom of the gastric ulcer in so many of the female cases of this malady, as to require a special consideration.

There are no data which would entitle me to make any definite estimate of the frequency with which the presence of this symptom coincides with the existence of the ulcer. But I have found reason to conclude that, on the whole, regular menstruation is far more common than is generally supposed. This fact is quite in consonance with what my inquiries have revealed respecting the total numbers of males and of females past the menstrual epoch, and not arrived at puberty, in whom the lesion has been detected by careful necropsy.

Further, even in the female during this epoch, a careful inquiry seems to indicate that this symptom associates itself with different groups of the lesion, in very different degrees.

It is in the chronic ulcer of middle-aged women that the catamenia are least affected. Many of the cases in which the ulcer has lasted for ten or fifteen years are recorded to have menstruated regularly; some even profusely. Indeed, in some the malady has lasted throughout the whole menstrual epoch of life, without exercising any appreciable influence on that function.

The coincidence of amenorrhœa with copious hæmorrhage from the ulcer is certainly more frequent. But in most of these cases, the relation between the two symptoms seems to be a very natural and obvious one. The amenorrhœa not only follows the hæmorrhage, but is caused by it; just as it would be ensured by any other serious hæmorrhage, or by that drain of nutritious fluids which pregnancy or lactation imply. In other instances, the amenorrhœa precedes the hæmorrhage. But since hæmorrhage is not more frequent in these cases than in cases of chronic ulcer in general, there is no ground for asserting the efficiency of suppressed menstruation as an independent cause of the bleeding. In like manner, there is rarely any connexion between the date of the hæmorrhage and the menstrual period. And finally, whatever has been said by authors respecting the liability of the gastric ulcer to give rise to a periodical hæ-

morrhage that forms a vicarious menstruation, I do not know a single well-authenticated instance of the kind on record.*

There is, indeed, but one group of gastric ulcers with which amenorrhœa seems to have any frequent or direct relation—viz., the perforating ulcers of the young female.†

In speaking of symptoms collected for the first time after the death of the patient (as has happened in many of the scattered cases of this kind which I have collected from different sources), there is so little hope of accuracy, that I do not think it worth while to state exact numbers. It may suffice to say that the majority of these cases exhibit scantiness or absence of the menses as one of the most prominent features of their history; that in many of them the amenorrhœa was accompanied with a state of pallor and anæmia, which was (somewhat rashly) termed chlorosis; that some, however, menstruated regularly and copiously; a few profusely; while a few had never arrived at puberty.

With the latter of these statements we may connect an allusion to a still more frequent condition of the same kind. Many of the so-called cases of amenorrhœa and chlorosis are instances of delay in the appearance of the menses, rather than of their suppression or interruption.

The age of many of the female subjects of these perforating ulcers corresponds to what we should expect from such facts. It is one which closely approaches to the average epoch of puberty, and the year or two immediately following; but which does not exhibit those fluctuations above and below this average age which would be requisite to assign it to the exact access of puberty itself. Still, the coincidence between the amenorrhœa and the ulcer is an unquestionable fact. And the first question concerning this fact suggests itself in the form of an alternative:—Does the amenorrhœa cause the ulcer, or the ulcer the amenorrhœa?

The first of these two questions I think we must answer in the negative: not only because the ulcer is present in the male sex, and in the neuter monster,‡ at the same age, as well as in the female at all other ages, but because, even in the female at this epoch of life, the exceptions

* The only instance I know of which approaches the characters of a vicarious menstruation, is one mentioned by Cruveilhier (Anatomie Pathologique, fol. 1835, vol i.), apparently on the sole testimony of the patient some years after. A periodic hæmatemesis replaced the menses "when these failed to appear, which happened often enough." It was accompanied by the expulsion of membranous tubes from the bowels, was unaccompanied by pain; and did not prevent the patient from working in the open air. Without more explicit information as to the date of the hæmorrhage with respect to the expected menstruation, the length of the menstrual periods and of the longest exclusively vicarious flux (its frequency alluded to seeming rather to militate against its duration), this case is hardly to be relied on. With it, however, I may mention another case, where there was no hæmorrhage, but in which it is vaguely stated that the pain in the abdomen was increased before and after menstruation. (New Medical and Physical Journal, for 1811, p. 89.) A similar instance in my own practice seemed, on close examination, to show, that the pain of the menstrual epoch was quite distinct from that of the ulcer being abdominal rather than epigastric in its site, and dysmenorrhœal in its nature (Association Journal for Jan. 12, 1856.) But in two or three other cases there has been an increase of epigastric pain at the beginning of the menstrual flux (Ibid, April 5, 1856.)

† This important fact we owe to Dr. E. Crisp, whose collection of cases, leading to this result, I have noticed elsewhere (British and Foreign Medico Chirurgical Review, Jan. 1856 p. 171). The meritorious industry of this gentleman has been so little recognised by some English authors on the gastric ulcer, that I am anxious to offer him this acknowledgment One or two inaccuracies which I have detected in his citations scarcely deserve notice, except in so far as they justify me in assigning to the connexion between the lesion and the amenorrhœa an explanation somewhat differing from his.

‡ Compare the January number of this Review, p. 171.

to the presence of the supposed cause are too numerous to be compatible with such a causation. Indeed, to the various cases of regular menstruation alluded to, we might plausibly add a large proportion of those in which menstruation had delayed its appearance, as well as all those in which puberty was absent. For surely many of the former would scarcely be instances of amenorrhœa, just as all the latter are certainly disqualified for this epithet.

In favour of an affirmative answer to the second question, or of the view that it is the ulcer which causes the amenorrhœa, we may point out that, in most of these cases, the dyspeptic symptoms which correspond to the establishment of the lesion have themselves preceded the deficiency or cessation of the menses; and that such an explanation, as it would receive no contradiction from the mere age of these cases, would find its parallel in the case of other grave constitutional disorders, which scarcely any pathologist would doubt to be the cause of the amenorrhœa by which they are frequently accompanied. A good illustration of this kind of suppression of the menses may be found in the tuberculous cachexia which often selects this epoch of female life as the period of its fatal attack; and which, though often associated with chlorotic symptoms, can generally be distinguished from true chlorotic amenorrhœa by a careful physical examination, aided by an accurate inquiry into the family history of the patient.

A careful observation of the details of that constitutional state which accompanies the amenorrhœa of these gastric ulcers, affords some confirmation of the above view. That state it is customary to speak of as "chlorotic." But I have never yet seen an instance that would suffice to establish the pathological identity of the cachexia present in this group of gastric ulcers with that of true chlorosis; nor do I know of any authentic records of such a case. The differences of the two states are, indeed, essential. The cachexia that attends the ulcer, even when best marked, is devoid of every characteristic symptom of severe chlorosis. The pallor, even where extreme, offers no trace of that greenish hue which the very name of chlorosis (χλωρὸς, *green*) connotes. The dyspnœa on exertion, and the soft bellows-sound, are much less distinct. And lastly, there is little or no œdema of the subcutaneous areolar tissue.*

As the age of these subjects of the gastric ulcer advances, it is not uncommon for the amenorrhœa to cease, all the other symptoms of the lesion remaining unaffected. In rarer instances, the so-called chlorosis also diminishes and disappears. But the most satisfactory proof that the amenorrhœa is not in any sense the cause of the chlorotic symptom, is afforded by some still rarer (though well authenticated) examples, in which the ulcer has been attended by a marked degree of this cachexia, without any interruption whatever to regular and copious menstruation.

But it is obvious that the above view does not at all explain the connexion between amenorrhœa and perforation; much less the fact that the ulcer is more liable to affect the menstrual function at this age of female life than at any other. Nor do I think that there are sufficient materials for such an explanation at present accessible to the pathologist. It would,

* This paragraph is quoted (almost *verbatim*) from some remarks on a case which offers a good example of the peculiarities of this group of gastric ulcers, and which will be found reported by the author in the Association Medical Journal for Jan. 12th, 1856.

however, be easy to suggest that such a periodic hæmorrhage as that of the menstrual flux might well be more easily affected during the struggles of the constitution to establish and maintain it, than when the organism had become accustomed to its recurrence, or strengthened so as to be more indifferent to the loss of blood it implies.

A much less vague and conjectural influence appears to be exercised by this epoch of life on the characters of the ulcer itself. That its great liability to perforation is not due to the ulcerative process sharing in the vigour and activity of youth (as has been suggested by an excellent authority), must, I think, be evident, when we consider the true physiological meaning of vigour on the one hand, and ulceration on the other. Vigour of ulceration is indeed weakness of health; nay more, vigour of constitution, supposing an ulcer already present, would oppose its progress, and limit or diminish its ravages, would ward off perforation by thickening its margins, depositing lymph on its surface, and gluing the peritoneal surface of the stomach to some adjacent organ.

But it is more satisfactory to state a conclusive fact than to point out what seems an abuse of terms. And since there can be no reason for denying to the male sex the vigour and activity such a theory ascribes to youth in general, the fact that the increased tendency to perforation at this age is limited to the female, may spare us all further reasoning on the subject. We need only add, that all the peculiarities of the symptoms and appearances seen in this group of ulcers refer the above tendency not so much to any special activity of the ulcerative process; as to the absence of that inflammatory reaction by which its destructive advance is often checked, and its worst effects warded off.

The cachexia generally associated with the ulcer at other ages of life appears to have precisely the same import as that already suggested for the chlorotic state which represents it in the young female. Like the latter, it seems to be essentially not so much a symptom as a congeries of symptoms: a state that expresses the injury inflicted on the organism by a variety of causes. The wearing effect of long and frequent paroxysms of pain, the fatigue and inanition implied by frequent vomiting, the drain of frequent or copious hæmorrhage, and the loss of digestive power involved in the destruction of the stomach, and finally, the mere age of the patient—are circumstances every one of which is likely to share in producing the cachexia that is present.

As already intimated, it is probable that this cachexia, which is best marked in ulcers of tolerably long standing, and therefore in middle-aged or elderly people, corresponds to the chlorotic symptoms and the amenorrhœa above noticed as generally present in connexion with the gastric ulcers of the young female. And my own experience entitles me to presume that it is rarely altogether absent; that, contrary to what is generally stated on this point, a person suffering from a gastric ulcer scarcely ever wears an appearance such as any observant practitioner would mistake for that of a person in health. Nay more, I might add, that the physiognomy of the disease is so peculiar, that I have sometimes been forewarned of its presence by the mere sight of the patient's features in a crowded Out-patient room at the Hospital. With many characters that would often leave us in complete uncertainty as to whether the

cachectic aspect was due to ordinary chlorosis, to tuberculosis, or (in later life) to the cancerous diathesis, the sharp lines that long and constant pain, and partial starvation, have worn in a patient's face, sometimes afford what is almost a *differentia*, characteristic of this disease. At any rate, this peculiar expression of countenance is, on the whole, a safer indication than mere anæmia, emaciation, or exhaustion can alone afford.

The *perforation* which sometimes occurs in the course of gastric ulcer is notified by symptoms so intense and characteristic, as to require but a very brief description. After more or less distinct indications of the ulcer have existed during a variable period of time, the patient is suddenly attacked by violent pain in the epigastrium, which rapidly spreads over the belly. Its diffusion is accompanied by the appearance of all the ordinary symptoms of peritonitis; the wall of the belly becomes extremely tender to pressure; the patient assumes an attitude which relaxes the muscles of this part; there is an absence of the usual respiratory movement here, which is rapidly followed by great tumefaction of the abdomen, and tympanitic distension of the bowels. If a strict physical examination of the belly be insisted on, the peritoneal cavity will be found to contain fluid—usually those contents of the stomach which have streamed through the aperture in its coats, increased by the subsequent addition of an inflammatory effusion. The continuance of these symptoms generally destroys the life of the sufferer in from twenty-four to thirty-six hours; but death is often preceded by a period of comparatively painless collapse.

Rarely does the train of symptoms that follows perforation offer any marked deviation from the above type.

In many instances, however, a remarkable paroxysm of pain precedes the occurrence of perforation. This intense pain—the duration of which varies from a few minutes to several hours—is, I believe, generally due to a leakage of the gastric contents through that thin film of rotten tissue, to which, at this period, the coats of the stomach are reduced. In consonance with such an explanation, a more chronic pain of similar character has sometimes been found associated with a complete matting together of the stomach and all the neighbouring viscera by a large quantity of lymph, without any visible perforation of the coats of the stomach, or any escape of its contents.

The symptoms of those various modifications* of the process of perforation which were alluded to in treating of the pathology of the gastric ulcer, offer few peculiarities worthy of notice. Partial perforation, allowing of a subsequent repetition (or rather extension) of the accident, or leading to abscess, would of course be distinguished by symptoms which, though differing from each other in every particular instance, would yet offer the general features of being more local, more chronic, and less intense, than those of ordinary perforation fatal by general peritonitis.

There are other circumstances attending the accident which might almost be enumerated among its symptoms. As stated elsewhere, it almost always occurs after a full meal; and is often distinctly traceable to mechanical violence, such as coughing, sneezing, concussion or constriction of the belly. The sensations of the patient frequently verify the nature of the accident, by distinctly appreciating that something has given way in

* See p. 171 et seq., of the last volume.

the belly, and thus caused a gush of fluid that has lit up the agonizing pain which has followed it.

Ampliation, or enlargement of the stomach, as a result of the constriction produced by the cicatrix of a gastric ulcer, constitutes a variety of the malady which, of course, brings with it a special train of symptoms. Into these, however, my present limits forbid me to enter. It is enough to point out that their connexion with the ulcer is both infrequent* and indirect; that they are present in instances of ampliation from so many other causes, that it is only by a history revealing those general symptoms of the ulcer already described, that we could refer any particular case to this origin.

The *Ætiology* of the gastric ulcer has hitherto been rather a subject for conjecture than for any successful inductive inquiry. And the author cannot flatter himself that his own researches will place it in the latter category. But since the exposure of error is one step towards the detection of truth, he ventures briefly to sum up whatever fragments of information he has been able to glean in this respect.

There can be no doubt as to the physiological circumstances which predispose to this disease. Old age, privation, fatigue, mental anxiety, and intemperance are such frequent coincidents of its occurrence, that we are fully entitled to regard them as its more or less immediate causes in a large proportion (I think we might say a majority) of cases. Of these, the influence of advancing age seems, from my inquiries, to be that which is most distinct and indisputable, and which rests on the broadest numerical basis of facts. But that careful clinical study of the malady which my Hospital opportunities have afforded me, leaves me just as little doubt with respect to the influence of poverty and intemperance.

We have found reason to qualify the ordinary notion with respect to the influence of the period of female puberty, and its attendant disturbances of health, in the production of the gastric ulcer. At least, we have seen that this particular epoch predisposes, not so much to the occurrence of ulcer, as to a peculiar character and termination of the ulcer; that it is a want of reaction, resulting in a tendency† to perforation, rather than a proneness to ulceration, which our numerical data would entitle us to assert. And even should this opinion be hereafter modified by a larger series of cases than I have been able to bring together, there will still be such a preponderance in the total formed by the gastric ulcers of the male cases, and of the middle-aged and old female, as to exclude the above group from any general significance with respect to the ætiology of the disease.

* Infrequent, as not occurring much oftener than once in 100 cases; indirect, as implying in most instances a previous cessation of the ulcerative process, or healing of the ulcer.

† In my previous Essay I omitted to add any estimate of the proportionate frequency of perforation at different ages in a given number of ulcers. From a calculation founded on the tables there adduced (p. 170), we may infer that the general proportion of one perforation to $6\frac{1}{2}$ ulcers, or 13·4 per cent., is made up of the following proportions in the two sexes, at the undermentioned ages:

	At ages up to			
	30	50	70	90
Per-centage of ulcers fatal by perforation in both sexes	40	$16\frac{2}{3}$	10	$4\frac{2}{3}$
In the male only	23	23	17	7
In the female only	59	10	3	2

The specific diseases with which the ulcer has been seen to occur, seem to have far less influence than the above physiological conditions. Tubercle, pleurisy and pneumonia, syphilis, ague, and fever, are those which are most frequently revealed by the necropsy or the previous history of the subject of gastric ulcer. But the per-centage of tubercle in cases of gastric ulcer does not seem to exceed its average in all persons indifferently. And the same statement will probably apply to all the remaining disorders. It would seem, too, from my own inquiries, that these cases of gastric ulcer do not exhibit that family history of phthisis which we should expect, on the supposition of there being any real connexion between the two diseases, and which we certainly find, under similar circumstances,* in the ordinary form of tubercular disease.

Some have sought to explain the peculiarities of the gastric ulcer by the circumstances of stomach-digestion. Ulceration, say they, having once taken place, the ulcer is prevented from healing, and even increased, by those great and sudden alterations in size which the organ undergoes at different periods of digestion; by the chemical and mechanical irritation of the food; and by the solvent action of the gastric juice upon the languid tissues that form the periphery and base of the ulcer, or upon the scarcely-organized lymph that has been poured out in this situation.

But in respect to the ætiology of the lesion, such an explanation is useless. For it is of little use to explain what happens after ulceration has once taken place, when the very point to which we want to arrive is—Why ulceration takes place at all; and especially, why it singles out this particular organ? It may be said, however, that the extension or continuance of the gastric ulcer form its chief characteristics: that the cicatrices of ulcers are frequently found in the intestine; and that the main peculiarities of the ulcer of the stomach, as contrasted with the ulcer of any other part of the alimentary canal, consist in its long duration, its gradual enlargement, and its reluctance to cicatrize.

I should be sorry to throw any undue discredit upon an explanation possessing so much practical truth and value as this certainly does; but it is the duty of those engaged in clinical researches to subject to a strict scrutiny every view which is not already established on an irrefragable basis. Now, there are not only good grounds for presuming that we have no series of facts (such as the careful records of systematic necropsies would afford) sufficient to establish the propositions these statements imply, but all the information as yet at our disposal is such as to inculcate the greatest caution in receiving them.

The ulcer of the stomach appears to cicatrize, probably without any medical treatment directed to this end, in about fifty per cent. of its total numbers. It does not seem to perforate oftener than about once in eight cases. No doubt, the class of intestinal ulcers, as a whole, offers a larger per-centage of cicatrices, and a smaller of perforations, than the above numbers. But the truth of the above propositions assumes an amount of this difference, such as we have at present no right to assert.

* Careful inquiry among the same class of Out-patients has shown me a remarkably hereditary character of the pulmonary tubercle to which they are so liable. (Compare the Author On Life Assurance, 1856, p. 12: also the Report of the Brompton Hospital for 1849.)

Further, when we examine into the two classes of lesions which we group under the terms of "gastric ulcer" and "intestinal ulcer," we shall find reason to attribute still less value to the above limited numerical differences. For we have seen that the majority of ulcers of the stomach cannot, in the present state of our knowledge, be traced to any specific constitutional disease. While we know that a large majority of those intestinal ulcers we are now contrasting with them, merely form the local expressions of a general malady, by the nature, date, and duration of which they are themselves dictated and regulated. The typhoid fever reaches its term, and the exulcerated agminate follicles generally heal over; the attack of dysentery subsides, and its ravages in the large intestine are more or less repaired; the phthisical cachexia continues, and the ulcers it has produced also remain. Are there no grounds for suspecting that the ulcer of the stomach may be due to some chronic vice in the organism, which dictates its occurrence, duration, and cessation, like the typhoid, dysenteric, or tubercular states of the constitution in these cases? At any rate, is there not a sufficient difference between the two classes of gastric and intestinal ulcer to forbid the unqualified reception of any pathological theory which regards them as alike in all save their mechanical and chemical circumstances? Besides, it seems very easy to overrate the influence of the circumstances above alluded to as tending to retard the process of cicatrization in the ulcer of the stomach. Wounds of this viscus heal with great facility, not only in the domestic mammalia usually selected for the purposes of physiological experiment, but in the healthy human subject. Indeed, in both man and animals, all fistulæ of the stomach seem to progress naturally towards closure and cicatrization, although placed under conditions which appear to be much less favourable to such a result than those of ordinary wounds or ulcers. While, as already mentioned, the ulcer itself heals with a greater frequency than can be explained by any theory which makes the circumstances of the digestive process so principal or special a cause. Lastly, every organ is adapted to its circumstances, and is generally organized to resist any unusual exposure that these may imply.

The details by which the process of ulceration is commenced would probably throw some light on its causes. But unfortunately, these still remain almost unknown to us. Direct observation is almost impossible. Analogy has little value. And the scanty and imperfect conjectures that both of these sources afford, are scarcely even consistent with each other, much less susceptible of a common application to any single view of the ætiology of the gastric ulcer.

In respect to the former, however, we may notice that several necropsies have shown ulcers of the stomach associated with more or less circumscribed patches of marked congestion, ecchymosis, or extravasation, which have been regarded as the beginnings of similar lesions in the neighbourhood of these ulcers. A similar (though quite distinct) class of appearances, constituting the "hæmorrhagic erosion" of Rokitansky, is believed by him to represent the mode by which the ulcer commences. But, to say nothing of the rarity of these appearances in conjunction with the gastric ulcer, I do not think any one conversant with the irregularities that often affect the distribution of blood after death in the digestive canal, would

like to assert the first class of phenomena to be distinctive of commencing ulceration. And the last is not only partially open to the same objection, but appears to be quite a distinct disease from the ulcer, lasting an indefinite time without showing the slightest disposition to merge into it, and exhibiting a somewhat different train of symptoms.

In still more exceptional cases, we find another class of phenomena, concerning which we may repeat the doubts already expressed respecting the above hæmorrhages. A distinctly oval or circular depression has been found in the neighbourhood of an ulcer of the stomach; or the mucous membrane has exhibited an appreciable softening of the same size and shape—once or twice even in conjunction with a reddish or darkish tinge of discoloration. Here, again, we ask, what proof have we that these depressions or softenings would have become ulcers, or even that they existed during life? Analogy, however, though it does not answer this question with a definite affirmative, at least affords us a strong presumption. That ulceration of the duodenum, which often follows severe burns, and which I have carefully avoided hitherto introducing into these discussions, because its situation, cause, and appearances alike seem to me essentially distinct from the ulcer of the stomach, has been all but seen to begin in this way.*

Such facts and analogies perhaps entitle us to hazard a conjecture, with which we will end these somewhat vague and unsatisfactory guesses at truth. Not only is there every reason to deny the existence of any specific disease that can lay claim to the title of "*the ulcer of the stomach*," but all the varieties that affect the form, rapidity, situation, numbers, and terminations of this lesion, seem to find their parallel in the causation of the malady, both as regards the organism generally, and those first departures from the normal state which inaugurate the local mischief. In short, we have no more right to talk of *the* ulcer of the stomach than of *the* ulcer of the leg; no more reason to assume an invariable commencement of the gastric ulcer by hæmorrhage, or by softening, or by a submucous deposit of lymph, than we have to restrict the beginning of what is evidently a similar process of destructive absorption in the limb, to an ecchymosis, a pimple, a superficial abscess, a burn, or a varicose vein. And just as our every-day experience assures us that external or cutaneous ulcers may begin by either of these lesions may have, that is, either of them as its immediate and conditioning cause, and may yet retain a general identity of that ulcerative process to which they are chiefly due—so not only do these very facts afford us fair grounds for supposing a similar diversity in the case of the ulceration that affects the stomach, but all that we have been able to glean respecting it confirms this analogy. The variety of diseases with which it appears to be connected, the equally numerous and diverse physiological conditions that favour its occurrence, can only thus be explained. Ague, fever, or the vascular disturbances of female puberty, might, perhaps, be supposed to facilitate or cause ecchymosis. But to convert this effusion into an ulcer would require a process of destructive absorption that no mere extravasation would explain. While the influence of old age, or privation, or fatigue, which throws little

* Transactions of the Pathological Society, vol. i p 258. Those who know Mr. Prescott Hewett's merits as a morbid anatomist will excuse me if I regard any observation of his as very unlikely to overstate the distinctness of the appearances present, or to confound the effects of disease with the sequelæ of death.

or no light on the precise local change that ushers in the ulceration, exactly concurs with the efficacy of these circumstances in the production or promotion of ulceration elsewhere. In like manner, to grant that the circumstances of digestion often retard the healing of a gastric ulcer — and the marked effect of treatment specially directed to these circumstances proves no less—is to concede nothing more than what, *mutatis mutandis*, we may verify for an ulcer of the leg, in which the same result is equally under the influence of such physical circumstances as posture, pressure, and the like. Nay more, the analogy not only applies to these details, but appears to illustrate the remarkable fact elicited in my former Essay. Here I found reason to conclude that the epoch which immediately follows the access of puberty in the female imparts to the gastric ulcer a peculiar character, best defined as a more or less complete absence of the inflammatory reaction that generally engages the base and margins of the lesion. And the large experience of Mr. Critchett has observed a similar character in the ordinary cutaneous ulcer at the same period of life; associated, too, with an analogous cachexia, and an equal disturbance of the menstrual flux.*

Diagnosis generally.—From what has been already stated with respect to the great varieties to which each of the symptoms of gastric ulcer is liable, we might, on merely arithmetical grounds, deduce the infinite modifications that would result from their combination. Of these it is no exaggeration to say, that they make each case of ulcer of the stomach unlike every other. And they especially suggest two questions respecting the diagnosis of the malady. (1.) What is the minimum of evidence that will justify us in affirming the existence of an ulcer of the stomach during life? (2.) What are the diseases with which it is most likely to be confounded?

A specific answer to the first of these two questions it is impossible to give. But I am inclined to think that nothing less than all the chief symptoms enumerated entitle us to pronounce a decided opinion. In other words, unless the pain possess the characters attributed to it, unless this pain be accompanied by vomiting, and unless there be evidence of hæmorrhage having occurred in the course of the malady, there is no sufficient basis for a definite diagnosis of the existence of a gastric ulcer. The date, duration, and frequency of the pain chiefly indicate some morbid condition of the mucous membrane of the stomach. The vomiting adds, that this disease implies great irritation of the nervous centres connected with the organ. And it is reserved for the hæmorrhage to show that the disease is such as to involve an absolute breach of continuity in the structure of the stomach.

But I have not the slightest doubt that an absolute enforcement of this rule of diagnosis would lead us to overlook a vast number of cases; and might thus be the occasion of grievous errors in practice. In point of fact, beyond the limits of secure diagnosis, there are a large number of cases in which we may justifiably entertain strong suspicions that the symptoms are due to this lesion.

* Critchett on Ulcers of the Lower Extremity, p. 107 et seq. London. 1849. The resemblance of the two lesions is completed by the facts, that in both the ulcer precedes the amenorrhœa, and is aggravated at the menstrual periods. (Compare pp. 170, 171 of this Essay.)

Here (as usual) I am desirous to be understood as speaking chiefly of my own clinical researches. But though I dare not lay much stress on the negative evidence derivable from the symptoms recorded in many hundreds of cases of perforation or hæmorrhage scattered through various journals,—for in a large number of these there may not have been such rigid and minute investigations of symptoms during the life of the patient as to justify us in denying the presence of all indications of disease save those mentioned,—still it is probable that many of them afford strictly accurate records of the dyspeptic ailments that have preceded the fatal attack. And hence it is very possible that of the numerous cases mentioned in which more or fewer of the above symptoms are not recorded, some are instances in which they were really absent.

But much more trustworthy evidence of such irregularities in the train of symptoms that characterize the gastric ulcer, is constantly being brought under my notice in Hospital practice. As might be expected, a moderate hæmorrhage readily escapes the notice of both the patient and his medical attendant. And even where the former habitually inspects the stools, or the physician calls in the aid of the microscope to an examination of any suspicious egesta, the irregularity of its occurrence may baffle all attempts to verify it for months together. In like manner, the vomiting seems to be sometimes, though much less frequently, absent from the history of the malady, during a great part of its course, or merges into a trifling regurgitation after meals, such as we hardly dare consider its representative.

It is of course impossible to define the precise degree of suspicion that ought to attach to a case in which the evidence of gastric ulcer was rendered imperfect by the absence of either of these symptoms, or of both simultaneously. But weak as such a suspicion often is, I am disposed to think that in every instance in which we find long or aggravated dyspepsia seriously affecting the general health, and associated with pain and tenderness in the epigastrium, and pain in the interscapular region, increased immediately after the ingestion of food,—there we ought at least to keep steadily before us the possibility of a gastric ulcer; as a possibility which, even if it falls far short of a definite diagnosis of this lesion, is yet sufficiently important to dictate the whole plan of treatment. And these remarks will especially apply to such symptoms when they occur in connexion with amenorrhœa in young females who have lately attained the epoch of puberty. Here the absence of hæmorrhage, and the little attention such persons often give to their dyspeptic symptoms, sometimes conspire to obscure the diagnosis: even where a careful inquiry into the history of the malady, and a sedulous examination of the epigastric region, afford us reason to suppose that the patient is in imminent danger of death by perforation of the stomach.

The above observations render it unnecessary to dilate upon the means by which we should generally distinguish between dyspepsia and gastric ulcer. In a great majority of cases, there is little difficulty in deciding which of the two maladies is present. But in some cases the distinction is by no means easy. And there are good reasons for conjecturing, that of all the Protean forms which dyspepsia may assume, that called the morbid sensibility of the stomach is the one which is most likely to

include cases of ulcer; or, in other words, if really independent of this lesion, is most likely to be mistaken for it.

To distinguish between ulcer and cancer of the stomach is generally just as easy: occasionally, just as difficult or impossible. As a rule, the symptoms of the cancerous lesion only appear at a comparatively late period of the malady, when the pylorus forms a hard moveable tumour, that can be easily felt from the exterior of the belly. Prior to this, the pain is either absent, or if present, has a lancinating character, and a time of appearance that belongs rather to the later stage of gastric digestion, than to the few minutes that immediately follow deglutition. The vomiting of the same stage of the malady often exhibits an analogous difference. The matters thus rejected will occasionally, to the practised microscopist, afford valid evidence of the presence of malignant disease in the cancer-cells they contain. The hæmorrhage is rarely excessive, and (ulceration being of comparatively late occurrence in gastric cancer) is often limited to the last few weeks of life. And lastly, the comparative duration of the two maladies, the age of the patient, or his cachectic aspect, often aid the diagnosis.

But not only do hardly any of these characters possess much independent value, but even several of them in combination may leave the question undecided. The gastric ulcer may come on as rapidly, and destroy life as quickly, as cancer. A large proportion of its subjects are middle aged or old. Many of these, too, have a cachectic aspect, such as is often easily mistaken for that of cancer; sometimes quite undistinguishable from it. The hæmorrhage of the ulcer may affect the moderate amount, and the "coffee-grounds" appearance, it ordinarily offers in cancer. The pain may be intense; and the access of its paroxysms may be habitually delayed until much more than the ordinary date after ingestion. Finally, the lymph by which an ulcer adheres to the liver may give rise to the production of a tumour, which is capable of being felt through the wall of the belly. And unlikely as it may seem that these separate contingencies should all combine to obscure the diagnosis of any single case, such instances do from time to time occur. Indeed, the reader may be referred to a late number of the 'Association Journal'* for an instance of this kind in my own practice; where though, from being engaged in the express study of these gastric diseases, I naturally gave the utmost attention to every feature of such a doubtful case, yet it was only at the necropsy that I could satisfy myself whether the ulceration I had diagnosed some months before was or was not malignant.

Art. II.

On the Proximate Cause of Functional Action. By J. Hinton.

The actions which take place in the animal body naturally divide themselves into two classes—the nutritive and the functional; or those which are concerned respectively in the formation of the organs and their use. In some instances, it may be difficult to draw the exact line at which nutrition ends and function begins, but for the most part the distinction is clearly defined, and theoretically the separation of the two forms of

* Association Journal for 1855, p. 1107.

action is always easy. There are three forms of function: nervous action, muscular contraction, and secretion. Taken in a large sense, these divisions appear to include all the active functions known to exist in the human body.

In the following remarks, no explanation will be attempted of the phenomena of nutrition: accepted for the present as ultimate facts, they form rather the basis upon which it will be sought to found a consistent theory of the cause of functional activity.

Little doubt can be entertained that the force which is operative in the vital processes is but a peculiar manifestation of the same force (or forces, if there be more than one) with which we are familiar under other names, as regulating the phenomena of inorganic nature. But although thus, in its origin, one with the other physical forces, the peculiarity of the conditions under which it exists in living bodies imparts to it specific properties, to designate which the term *vital* is employed. One of the most characteristic of these peculiar modes of action of the vital force, is the opposition which it presents to the operation of those forms of force which are termed *chemical*—an opposition not of essential nature, but of special direction. The vital force (or, as from this point of view it might be called, the vital affinity, for the sake of bringing out more clearly at once the relation and the contrast) controls and holds in abeyance the chemical tendencies of the matter in which it subsists.*

From the state of chemical tension thus arising, it results that there exists in all living matter a constant tendency to change. No sooner are the conditions requisite for the manifestation of vital properties withdrawn, than chemical affinity resumes its sway, and decay commences. Even during life the same process is continually going on. The tissues waste, and are renewed, and waste again.

A certain connexion between this waste or disintegration of the tissues and the functional activity of the body in which it takes place, is universally admitted. Yet the relation which subsists between them is by no means satisfactorily established. For the most part, the activity is held to precede and cause the waste.

"Discharge of function, *consequent degeneration* absorption, and replacement by new structures."†

"In the history of a cell there are three stages,—that of its growth, of its decay, and the intermediate one of its functional activity, which is dependent upon the first, *and which causes the third.*"‡

"We may look upon the death of such cells (the muscular tissue), whose term of life might otherwise have been considerably prolonged, *as the result* of the expenditure of their peculiar modification of force under the guise of mechanical power"§

In this representation it appears to me that the relation of cause and effect is inverted—that the existence of a controlled and subjugated tendency to chemical change in living bodies is the origin of all the capacity for functional action which they display, and that the disintegra-

* There can be no difficulty in conceiving forces essentially the same acting thus circumstantially in opposition. Innumerable instances will occur to the mind in which heat, for example, opposes chemical affinity, or gravitation itself raises or suspends a weight.

† Mr. Paget: Lectures on Surgical Pathology, p. 131.

‡ Dr. Bucknill: British and Foreign Medico-Chirurgical Review, No. 29, p. 226.

§ Dr. Carpenter: Human Physiology, p. 109.

tion of their tissues is not a "result" or "condition" of their activity, but rather the moving spring and source of that activity itself.

The life of the body being one, its functional power must be one also. Widely as they may differ in their immediate form and object, the functions, when regarded in relation to their origin, may not be isolated from each other. They are common products of a single power, which requires to be investigated at once in all its modes of action. Hence, probably, the want of success which has attended the various attempts that have been made to trace the physical causes of separate functions. But, on the other hand, much of the obscurity which attaches to the ideas of life and the vital force appears to have arisen from the attempt to include under one denomination, and to refer to one mode and development of force, phenomena of diverse, and indeed opposite, characters.

Broadly as the line of demarcation is drawn by nature between those processes by which the living organism is built up and maintained, and those which involve the death and disintegration of the tissues in which they occur, the prevailing tendency of physiological speculation has been to include both series of actions under one name, and to refer them to the immediate operation of a common power. They have been termed indiscriminately *vital actions*, and adduced without distinction as instances of the direct operation of the vital force.

Thus Liebig says: "The active or available vital force in certain living parts is the cause of the mechanical phenomena in the animal organism."*

And Dr. Carpenter thus expresses himself: "The contraction of any muscle upon the application of a stimulus must be attributed to an exercise of *vital force* engendered by previous acts of nutrition."†

And again, speaking of muscular and nervous action, he says: "We are entitled to affirm that each is a peculiar modus operandi of the same force as that which is concerned in cell-formation."‡

According to this view, the vital force is made the direct agent in actions essentially different. Hence arises the impossibility of defining it; for while the words are so used it is surely in vain to seek to attach to them any signification more definite than that of a general expression for all the changes which take place in a living body. Any term similarly used would become equally obscure and unsettled. By thus including in one category actions so opposed as function and nutrition, the phenomena of life are placed in an attitude of irreconcilable variance with those which pertain to all other branches of physical science. The fatal error has been to overlook the fact that two forces (or modes of force) are at work in the living body. It has not been perceived that the *chemical* affinities of the animal organs constitute a source of power co-equal with, and precisely measured by, the power of the vital force. The work of two agents has been assigned to one. If now the omission be supplied, and the vital and chemical forces be recognised as the two forces of organized matter—the former as the resistance, the latter as the resisted force, and therefore the force available for action—a large part of the obscurity

* Organic Chemistry of Physiology and Pathology, p. 221.
† Human Physiology, third edition, p. 476.
‡ Philosophical Transactions, part 2, p. 737. 1851.

which envelopes the theory of vital action is at once removed. An uniformity of principle is seen to prevail between the laws of the organic and inorganic worlds, and the facts hitherto so intractable arrange themselves without difficulty in accordance with some of our most familiar conceptions.

Bearing in mind that no explanation is offered of the nutritive processes in the living body, it will be seen that upon the theory propounded there is a perfect analogy between the animal body and a self-acting machine.

In both there exists a mechanism adapted to the performance of certain defined actions, and a reservoir of power or force by which that mechanism is kept in operation. In both, the source of this power is essentially the same. In living bodies one tendency of matter, its chemical affinity, is held in check; in any machine that is to manifest a capacity for action, art must bring into a like condition of restraint some tendency of matter, either the same or similar.

In the simple instance of a clock, the gravitation of the weights, controlled by an adapted mechanism, is the power which effectuates its functions—the revolution of the hands, the striking of the hour. In the watch, the restrained elasticity of the spring holds the same relation. The steam-engine owes its power to the repressed expansiveness of the vapour. There is no instance, indeed, of an artificial accumulation of force or capacity for action that does not depend upon this principle. Matter restrained from the fulfilment of any of its natural tendencies affords power; the removal of the restraining force, permitting the play of the tendencies so controlled, produces action; which action may be made to subserve any purpose by suitable modification of the resistance, and the employment of an adapted mechanism.

In this respect the organic and inorganic worlds obey a common law. Organization gives capacity for action only by virtue of the resistance it presents to the chemical forces; these chemical forces, acting under definite limits, and in connexion with various structures, being the true sources of all functional activity. A living body is a divinely-made machine, constructed, indeed, with a marvellous delicacy, perfection, and complexity, and depending upon a power, the vital modification of force, which it is wholly beyond our skill to imitate or comprehend, but still involving in its *working* no other principles than those which we every day apply, and see to regulate the entire course of nature.

For the inorganic world furnishes abundant instances of the same balancing of forces resulting in a similar activity or capacity for action. The term *irritability*, in so far as it denotes a capacity for responding to stimuli, confined hitherto to organized structures, might with perfect accuracy receive a more extended application. It exists in whatever form of matter there is found the same powerful tendency to change of state with which it is associated in living bodies. Thus, in the chloride or iodide of nitrogen the slightest touch induces an explosion. In the case of gunpowder, the tendency to change in which is less energetic, the chemical affinities of the materials are brought into action by the momentary application of intense heat. In the same way a solution of certain salts, when the cohesive force is barely counterbalanced by the sol-

vent power of the water, will assume the crystalline form upon the gentlest touch, or the mere passage of a vibration. The slightest scratch causes unannealed glass to break.

In these instances—and very many more might be adduced—it is surely correct to say that action ensues on the application of a stimulus; and in them all it is obvious that the action is immediately due to pre-existing and restrained tendencies to change of state. The stimulus is only in a secondary sense the cause of the phenomenon, and evidently determines it by removing the condition which forbade the previous operation of those tendencies. In all such cases the modus operandi is the same as that of the mechanisms previously referred to, and they are precisely analogous to the simpler contrivances in which a suspended weight is made to fall upon the disturbance of its equilibrium by slight causes.

If the doctrine of the correlation of the physical forces be applied to material actions or changes of this class, it becomes at once apparent that the correlated force is neither the resistance nor the stimulus, but the controlled or latent tendency to change.

Thus, e. g., the application of a certain amount of heat to a magnet suspends its attractive power. If therefore, to a magnet sustaining a mass of iron sufficient heat be applied, there results an action—the fall, namely, of the iron to the earth, the cause of this action being the gravitation which the magnetic force had previously been exerted in controlling. It might be said that the gravitation is converted into motion; it would never be proposed to attribute the motion to a conversion either of the magnetic force or of the heat into mechanical force. But in respect to the animal functions, this very error has been committed; for in the illustration above cited the magnetic attraction represents the vital affinity or force, the gravitation the repressed chemical affinities of the living tissues, the heat the stimulus, and the fall of the weight the function.

Many arguments may be adduced to show, that while the Correlation Theory affords a consistent and beautiful expression of the relation which exists between the forces of the external world and the developments of the vital force in the growth and nutrition of the body, it is entirely misapplied when it is proposed as an explanation of the connexion of the vital force with functional activity.

In the first place, this view entirely ignores the balanced state of the forces in the animal economy, and the accumulation of power arising from the repressed chemical affinity, which it regards merely as operating, after the vital force has discharged the function, in reducing to simpler compounds the devitalized tissue. Surely this is utterly opposed to all we know of the economy of force which prevails throughout nature, and pre-eminently in the living body, in which no power, how subordinate soever, or apparently trivial, is ever wasted.

It is unquestionable, that in this state of equilibrium of the chemical and vital forces there exists an arrangement by which great results might be accomplished. Everything is prepared for the exhibition of a large amount of power by the mere permission of the play of chemical affinity. Would it not be a gratuitous squandering of resources that such a capability for action should be turned to no account?

2ndly. To suppose a conversion of the vital force into functional action,

is to set aside an actual and sufficient cause in favour of one that is entirely hypothetical. The state of chemical tension in the animal body, and the co-existence of chemical change with functional activity, are admitted facts: that this chemical action in the tissues gives rise to the external manifestations of function, is an inference as simple as that the chemical change among the particles of gunpowder is the cause of its explosion. How, then, are we justified in assuming the existence of another process, hard to conceive, and impossible to demonstrate?

3rdly. The theory in question, while it rejects a cause so natural and obvious, in reality involves the idea of an effect without any adequate cause at all. No intelligible relation of cause and effect can be shown between the stimuli which excite the functions and the conversion of force which they are supposed to cause, or for which they "supply the condition." No proportion is maintained between the amount of the stimulus and the amount of force converted. In what way, for instance, can gentle pressure on the thumbs of the frog, during the season of coitus, produce a conversion of the vital force of nearly all the muscles of the body into an energetic contractile action?

4thly. Waiving all theoretical objections to the view of the correlation of vital force and functional activity, it may be remarked that the facts do not agree with the principles of that doctrine. The "material substratum" is wanting. In the conversion of the vital force of a muscle into mechanical force, for example, there is no change of the matter in which the force subsists. The conversion supposed is precisely such as would occur if a heated body were suddenly and without adequate cause to lose its heat, and manifest electricity instead, or shoot into spontaneous motion. The view propounded by Liebig—viz., that the vital force which is converted into mechanical force in muscular contraction is not that of the muscle itself, but may be derived from any other part of the organism, and conveyed to it by the nerves—would be more accordant with the terms of the theory, but we know experimentally that it is not correct.

5thly. The vacillating language used in reference to this part of the subject, by those who have most successfully applied the doctrine of correlation to vital phenomena, betrays the unsoundness of their position.

"Muscular contraction," says Dr. Carpenter, "may be regarded as proceeding from the expenditure or metamorphosis of the cell force, which ceases to exist as a vital power, in giving rise to mechanical agency." But speaking of the external stimuli of muscles, he adds: "These agencies are concerned in occasioning that metamorphosis of living organized tissue into chemical compounds, whereon the development of the muscular force seems to be immediately dependent."*

Are not two different origins here assigned to muscular contraction? Again, Dr. Carpenter observes (p. 747), "We are, then, to regard the *nervous*, electrical, and other stimuli under whose influence the muscular force is called forth, less as the immediate sources of that force than as furnishing the conditions under which the vital force, acting through the muscle, is converted into the mechanical force developed in its contraction."

* Philosophical Transactions, part ii. p. 746. 1850.

But at p. 745, we read: "The *nervous* force appears convertible into motion through the medium of the muscular apparatus."

With regard to the nervous force, Dr. Carpenter writes as follows: "We find only one kind of tissue serving for the generation and transmission of nervous power, this alone affording the material substratum through which the *vital force* can manifest itself as nervous agency." And again: "Nerve force *which has its origin in cell formation* may excite or modify the process of cell formation in other parts." (p. 743.) But, on the following page, he argues, that "all the facts that have been adduced in support of the identity (of the nervous force and electricity) will be found readily explicable on the idea of their correlation or mutual convertibility."

Can the nerve force be both a manifestation of vital force and a result of the conversion of electricity? Can it have its origin at once in cell formation *and* in a galvanic current? And yet, further, are there not the same reasons for holding that the electrical stimulus only furnishes the conditions under which the vital force is converted into the nervous force, as exist in respect to muscular contraction?

Even Liebig's perspicuity fails him upon this subject. In his observations On the Phenomena of Motion in Living Bodies, he writes thus: "All experience proves that there is in the organism only one *source* of mechanical power; and this source is the conversion of living parts into lifeless amorphous compounds."*

But at p. 220, "As an immediate *effect* of the manifestation of mechanical force, we see that a part of the muscular structure loses its vital properties, its character of life."

Is not the same change thus made both cause and effect?

The last writer on this topic, Dr. Reynolds, in an able article On the Objects and Scientific Position of Physiology,† is not more definite in his language. Compare the following passage:—"The partial disintegration of the tissues (of the muscular and nervous systems) is one condition or *source* of their action." (p. 112.) "We have therefore to regard these animal properties (sensibility and muscular contraction) as functions of the *vital force* inherent in the cell, and as constituting two of its special endowments." (p. 118.)

In the passages above cited—and many more of the same character might be adduced—two contradictory ideas appear to have been struggling in the writer's mind, and alternately giving the colour to his language. One is, that motion, or nervous action, as the case may be, is a direct expression of the vital force; and the other, that it is the result of the chemical disintegration of the muscular or nervous tissues. Owing to this cause, the words used virtually assert that the retrograde metamorphosis of the tissues, or their conversion into lifeless compounds, is a result or manifestation of the vital force, which is in its very terms a contradiction.

To these considerations it may be added, that to affirm the function to be the result of the accompanying disintegration, is to adopt the negative side of the argument. It enables us to reject altogether *sensibility* and *contractility*, as separate properties of the nervous and muscular tissues,

* Op. cit., p. 242. † British and Foreign Medico-Chirurgical Review, No. 31.

apart from their known tendency to chemical change. And no principle in science is better grounded than that nothing may be assumed to exist without a proved necessity.

The substance of what has been advanced may be briefly stated thus. Dynamically considered, the changes which take place in the inorganic world are divisible into two classes—those which directly result from the application of some new force to the matter in which they occur, and those which ensue from pre-existing tendencies to change when some force previously operative is neutralized or overcome. The former class of material changes are characterized by an absolute proportion between the force applied and the resulting action; the latter are distinguished by their spontaneity, or the disproportion (often extreme) between the apparent cause and the result.

The endowments of living beings embrace both these forms of action. The first is seen in the processes of nutrition, development, and growth, the forces engaged in which are truly correlated, as Dr. Carpenter has most ably shown, to other forms of force. The changes in which function consists exemplify the second, being effected by the chemical affinities of the elements of the tissues, when the vital resistance is in definite manner and degree diminished.

Treating the question thus on abstract grounds, it can hardly be denied that the view of the vital functions above propounded possesses great simplicity, and by virtue of its wide analogies, a certain amount of *à priori* probability. It aids in reducing to the smallest number "the assumptions which, being granted, the order of nature as it exists would be the result." But it cannot on such grounds claim acceptance, unless it be capable of an unstrained application to all the phenomena which come within its scope. It would almost appear, indeed, to be so natural an interpretation of the facts of animal existence, that had it been the true one, it could hardly have been overlooked or rejected, and that the class of functional actions must have presented characters which, indicating the direct agency of the vital force, forbade them to be grouped under so simple an expression. I shall proceed, therefore, to an examination of some of the leading facts connected with the animal functions, and inquire:—

1. How far the actions of the nervous system may be interpreted upon the principle suggested. From such an inquiry it is of course necessary to exclude altogether the phenomena of thought and volition, viewed in their psychological relations. Of the mysterious process by which a material change in the brain awakens a perception or kindles a thought, we are entirely ignorant; nor can we form any conception of the mode in which the spiritual will communicates its behests to its obedient instrument. Whatever theory be adopted of nervous action, these relations must remain equally inscrutable. Confining our attention, therefore, to those operations of the nervous system which are strictly physical in their character, it may be remarked that all the stimuli which excite them are adapted to bring into activity the repressed chemical affinities of the organic elements. Thus the nervous force is called into action by mechanical irritation, or motion in whatsoever form applied, by changes of temperature, by chemical reagents, electricity, light, or sound, and by the sapid and odorous properties of matter. It is hardly possible to perceive in these various

agents any property in common to which their influence upon the nervous system can with reason be referred, except the power they all (so far as they are known to us) possess of disturbing an unstable chemical equilibrium. They cannot all supply a force which is converted into the nervous force. They have no visible adaptation to cause such a conversion of the vital force. No analogy warrants the assumption that they can immediately produce a state of active polarity. But acting upon a tissue in which the affinities of the component elements are so delicately balanced, and the inherent tendency to chemical change so strong, it can hardly be otherwise than that they should overthrow that balance, and bring into play the latent and coerced attractions.

"In compounds in which the free manifestation of chemical force has been impeded by other forces," says Liebig, speaking of inorganic substances,* "a blow, or mechanical friction, or the contact of a substance the particles of which are in a state of transformation, or any external cause whose activity is added to the stronger attraction of the elementary particles in another direction, may suffice to give the preponderance to this stronger attraction, and to alter the form and structure of the compound."

That such an actual change of the composition of the nervous tissue does ensue from the action of the stimulus, is proved by the fact that the same stimulus will not reproduce the effect until after the lapse of a certain interval. This should not be the case if the stimulus merely induced a polar state, or itself assumed the form of the nervous force. The necessity of time for the renewal of the irritability is evidence of an altered composition.

Instances have been adduced from the inorganic world of the production of action in substances prone to change by slight mechanical irritation, which may be referred to as the analogues of the sense of touch. The senses of sight and hearing are susceptible of illustration by similar analogies.

To prepare a plate or paper for photographic purposes, it is only requisite to apply to it a suitable chemical compound, the elements of which tend to assume other relations, and of affinities so weak as to be overcome and neutralized by light. Thus prepared, the paper is called sensitive, and it would appear to furnish a very exact illustration of the process by which vision is effected.

The retina consists of matter prone to change. Its elements break up and enter into new relations immediately the vital force or affinity which holds them in their existing combinations ceases, or becomes impaired. What hypothesis can be more simple than that the luminous rays of the spectrum should have the power, to a certain extent, of neutralizing this delicate affinity, and thus causing, or, to speak more correctly, permitting, a definite chemical change to take place in the retina; just as the actinic rays, overcoming the affinities of the photographic salts, cause or permit a new arrangement of their elements?

The sense of hearing also admits of explanation by the application of the same principle. In the texture of the auditory nerve it appears that the chemical and vital forces are so balanced that the sonorous vibrations overthrow the equilibrium, and bring into activity, as in the case of light,

* Op. cit., p. 207.

the chemical affinity. An illustration of the nature of the action is furnished (if we may compare great things with small) by the fact mentioned by Rogers, that masses of ice and snow of considerable magnitude may be precipitated from the Alpine ridges by the sound of the human voice. The gravitation of the masses, and the resisting forces which maintained them in their places, being in such exact equilibrium, that even so slight a motion of the atmosphere suffices to give the preponderance to the former. This illustration, remote though it may seem, is valuable, as bringing clearly before the mind the essential character of the process which constitutes the animal function. For the stimulus in this case, the aerial vibration, palpably induces the resulting action, not by any direct agency, nor by a conversion of one form of force into another, but solely by disturbing the equilibrium of the counteracting forces, and neutralizing the resistance which opposed the force of gravity.

Such a change of composition in the nervous substance must tend directly, in conformity with all our knowledge of physical laws, to produce a polar state or force, corresponding in every respect with that which we term the "nervous force." The close analogy which exists between the nervous force and electricity, strongly confirms this view of its origin and nature. For we recognise chemical change, and especially the decomposition of compound bodies, by means of stronger affinities acting on their elements, as an invariable source of the electric force; and Mr. Grove has demonstrated its existence as a result of the changes which take place in the photographic process. In the living body, it would appear that the decompositions (if they may be so called) in which the exercise of function consists, give rise to a force—not electric, indeed—but of a peculiar though analogous character, inasmuch as the changes in which it has its origin, though analogous to those which take place in inorganic matter, are yet of a distinct and peculiar order. Thus regarded, the nervous force, in its relation to functional activity, may be defined to be a polar condition, or other molecular change in a nerve, akin to that which exists in a body conveying a current of electricity, and arising from a chemical change either in itself or in any of the tissues with which it is in relation. This change being the result of the chemical affinities of the elements of the tissues, which come into play when the vital resistance is diminished by any force which, so disturbing the equilibrium, is called a stimulus. I have said the nervous force may be thus defined in its relation to functional activity, because there appears to be much evidence that the changes which constitute the development and nutrition of the tissues also give rise to a force which, traversing the nerves, contributes materially to the energy of the vital processes, and more especially, perhaps, to the sympathetic development of various portions of the body, and the general condition of vigour which is denominated tone. This question, however, does not fall within the scope of the present paper, which relates only to those actions in the living body that are attended with a retrograde change of structure.

The nervous force, therefore, having its origin in chemical or anti-vital changes, must possess an especial adaptation for exciting changes of a similar character. Hence it is pre-eminently the excitor of function, causing in any organ to which it may be conveyed the same subordination

of the vital to the chemical affinity from which it sprang. To take another illustration from the eye. Light impinging on the retina determines therein a chemical change, which developes in the optic nerve the nervous force. This force causes in the brain an action of the same order as that in the retina. Hence again originates a nervous force, which, conveyed to the iris, causes yet a third time a chemical change, which is the source of its contraction.

That the nervous force, as excited by stimuli, is opposed to the vital affinity, and tends to the induction of changes resulting in the disintegration of the tissues, is rendered probable, not only by its relation to the functional activity of the organs, which is always connected with such disintegration, but also by various facts which show ulcerative or other destructive action to be the result of abnormal stimulation of a nerve, or even of the excessive application of the normal stimuli. An interesting case of this nature is mentioned by Mr. Paget, in which obstinate ulceration of the palm of the hand was caused by pressure on the median nerve, and which healed immediately the pressure was removed. Another case is mentioned by Mr. Simon, of ulceration accompanying neuralgia of the knee. Nor can such destructive effects be attributed rather to the withdrawal than to the derangement of the nervous force; for although ulceration may occur as the consequence of the division of a nerve, there is ample evidence that it is not due to the mere loss of nervous stimulus, but either to the "irritation" consequent on the division, or to the absence of necessary protection to the organ implicated; and that the abnormal stimulus is often the cause of the ulcerative process in these cases, appears highly probable from a case related by Mr. Simon, of disease entirely destroying the fifth nerve, in which the cornea of the affected eye had ulcerated and healed again.

The view of the nervous force which refers its origin to retrograde metamorphosis receives confirmation from various facts which, upon any other hypothesis, are difficult of explanation. Such are—

1. The increased proneness to functional activity which (with certain limitations) always coexists with diminished vital power, and is implied by the expression that *irritability is proportionate to debility.*

2. The phenomena of certain diseases: as tetanus arising from the disorganizing changes caused by a wound, in a debilitated constitution; or those cases of epilepsy in which the cause of the convulsion appears to be merely the mechanical irritation of spiculæ of bone pressing upon the nervous tissue, and the more permanent convulsive action connected with that retrograde change in the brain which is denominated red softening. And lastly, the fact that the mere destruction of the central ganglia, as by crushing or other mechanical violence, induces a vehement exhibition of nervous energy.

II. An examination of the conditions which determine muscular contraction will show them to be in perfect conformity with the principles laid down. The proposition affirmed being that the motor power of a muscle is simply an expression of the state of chemical tension in which it exists, and that its contraction is the immediate result of a change of composition ensuing whenever the vital state which maintains such tension is, within certain limits, thrown into abeyance.

When placed beneath the microscope, the ultimate muscular fibre is seen to contract first at any spot where it has been broken or otherwise subjected to injury. The slightest mechanical irritation, even the presence of the least particle of matter, determines a local contraction, as also do chemical reagents and water. The contact of the atmosphere, which we know, from the history of subcutaneous wounds, to have a lowering influence on the vitality of the internal tissues, excites irregular contractions on the surface of an exposed muscle.

In cases of protracted phthisis, or other diseases attended with exhaustion of the vital power and emaciation, contraction of the muscles arises with increased facility, and may be visibly excited by a light blow upon the muscles of the thorax.

And during vigorous life, the stimuli which best excite the action of the muscles are precisely those which most powerfully evoke their inherent tendency to change of composition. The nervous force has been shown to stand in a special relation to such change. Electricity, which as a muscular stimulus ranks second to it in power, stands first among the physical forces as a promoter of chemical change, and manifests its opposition to the vital force by the instant death which accompanies its excessive action; by the coldness, pallor, and depression of vital energy which follow its local application in a powerful form; and the more speedy putrefaction of muscles which have been electrified immediately before or after death.*

The phenomena of post-mortem contraction of the muscles are, perhaps, not strictly comparable with those of their living action. It may be doubted whether they are facts of the same order; but so far as the former are available for illustration of the latter, they entirely support the view that contraction depends upon a diminution of the vital resistance, allowing to a limited extent the play of chemical affinity.

The simplicity and adequacy of this theory are well exemplified by its bearing upon the dynamical problem involved in the motion of the heart. Of the various extraneous forces to which the maintenance of its action has been assigned, all have been rejected by Dr. Carpenter, who prefers to regard—

> "An alternation of contraction and relaxation as the characteristic and constant manifestation of its vital activity. . . . . Just as the Leyden jar," he adds, "may be so charged with electricity as to discharge itself spontaneously, so it is easy to conceive that a muscle may be so charged with motility or motor force as to execute spontaneous contractions."†

A few considerations will show that this hypothesis cannot be accepted as a correct representation of the action to which it relates.

For, in the first place, the motion of the heart or any muscle (as Dr. Carpenter himself represents the case) is not a *manifestation* of the vital force, but a *conversion* of it. And such conversion cannot occur without a preceding change in the conditions under which the force exists. To suppose it to take place spontaneously, is to suppose a material change to originate itself; an effect without a cause.

* The varying effects of electricity upon the muscles according to the direction of the current and other circumstances, are perhaps not yet entirely explicable upon any general principle. It is believed that they are not more difficult of explanation upon the view maintained above, than upon any other hypothesis.

† Human Physiology, p. 476.

Again, the words "motility or motor force" are most unhappily wanting in precision; and whether they be held to mean actual motion, or capacity for motion, the idea seems to be alike inapplicable.

The illustration, also, adduced by Dr. Carpenter does not assist his argument. In the Leyden jar, electricity received from without is accumulated by resistance, and transmitted when the resistance ceases, either being neutralized by the use of the discharger, or overpowered by the excessive accumulation of the resisted electricity. That is, as if a real momentum of motion were imparted to the muscle, and stored up within it by resistance, until it had accumulated to a sufficient intensity. But the heart, on Dr. Carpenter's view, is in no such case: no account is taken of any force resisted; the entire process is a continuous development of one force, suddenly altering its character and mode of operation.

The deficient element is the force which determines this sudden change from a form of action which builds up the living tissue to one that disintegrates and destroys it. The chain is broken at that point; but the recognition of the two forces which inhere in every part of the animal body, at once supplies the wanting link. The heart, like every living muscle, is charged with force, not motor or contractile, but chemical. The chemical affinity of its elements, resisted by vital or nutritive action, accumulates within it, creating a state of tension and proneness to action, precisely such as exists in the Leyden jar. The comparison is just, though incorrectly used. Muscular contraction from a stimulus is the analogue of the electrical discharge by means of metallic contact, in which the resistance is removed; and the spontaneous contraction of the heart is parallel to the spontaneous discharge which ensues when the resistance is too weak.

An adequate account of the facts appears to be conveyed by the following statement. In the muscular structure or nervous ganglia* of the heart, the chemical and vital forces are so balanced, that they assume a state of alternating activity. It might be said that the vital force exists in large quantity, but of low intensity. Hence, when, by the process of nutrition, the chemical affinity has been accumulated to a certain amount, it overpowers the vital resistance, and that chemical change which is the cause of contraction ensues. And the same series of changes continually recurs, because the vital state is constantly renewed. It is possible that the maturity of the cells which constitute the muscular fibre, being accompanied by a failure of their vital power, may give the occasion for the ascendency of the chemical force; but the phenomena of voluntary muscular contraction, and the fact that the heart's action is often more rapid in proportion to the debility of the vital power, seem opposed to such a view. The action may be roughly compared to the alternate formation and decomposition of the ammoniuret of mercury in the course of an intermittent electric current.

In the foregoing remarks, it has been assumed that the vital force is characterized by a varying intensity of action. In proof of this law, it is sufficient to refer to the normal succession of the sleeping and waking states. The heart may be said to wake and sleep with each recurrence of its beat.

* There are many circumstances which favour the idea that the action of the heart is dependent upon the ganglia contained in its substance.

With regard to the mode in which chemical change of the muscular tissue effects its contraction, nothing certain is known. There is no difficulty, however, in the conception of such a causal relation, since the production of mechanical force by means of chemical action is one of the most familiar of facts, and the muscular structure may, without any violence, be regarded as a mechanism adapted for the development of mechanical effects from slight changes of composition.

III. With regard to the process of secretion, there is ample evidence that it depends upon a modified exertion of the *chemical* affinities. The following facts may be referred to:

1. The lower composition of the secreted fluids. In the case of the great mass of the secretions, including those of a nutritive character (as the milk), this less vitalized constitution is evident, and the seminal fluid, there is reason to believe, is no exception. To what, e.g., but an exercise of chemical affinity can the formation of sugar by the liver be referred?

2. The dependence of the secretive action upon the same stimuli and general conditions as the other functions, and especially upon the nervous force.

3. Its promotion by the local application, or presence in the blood, of medicinal or other substances, the influence of which cannot increase, but must tend to diminish the vital resistance of the organs. It is not unlikely that in some instances the secretive action is normally maintained by the decomposing influence upon the gland tissue of substances, themselves in a state of decomposition, circulating in the blood.

4. An over-stimulation of secretion leads directly to destructive and anti-vital changes. Thus, as Mr. Paget has observed, the first stage of inflammation appears to be merely an increase of secretion. Salivation runs on to ulceration. One effect of destructive agents applied to the surface of the body, as a burn, severe pressure, or chemical irritants, is to induce secretion.

5. Professor Graham has rendered it probable that the passage of osmotic currents through animal membranes is dependent upon slight decomposing changes taking place in them.

6. Secretion may continue after death, being then analogous to the post-mortem contraction of the muscles.

The production of electricity and of light must be enumerated among the animal functions, but it will be sufficient merely to allude to them. There is no cause to which they can be referred with more probability than to chemical changes in the electrical and luminous organs. And the generation of electricity is known to be determined, like the other functions, by any stimuli which tend to overthrow the chemico-vital equilibrium, either in the organs themselves, or those portions of the nervous system which supply them.

The view of the vital functions advocated above has many important bearings upon special branches of physiology and pathology, which cannot now be enlarged upon. The great advantage which seems to result from it is the simplification it effects in the conception of the vital force itself. One whole division of what under other views is considered as vital action, being thus transferred to the domain of chemical agency, the idea

of the vital force stands out clear and distinct before the mind as the peculiar molecular action which forms and nourishes the living body. That is its nature; that its entire scope. Thus, by resistance, it accumulates chemical force, and furnishes the conditions under which THE FUNCTIONS—motion, nervous action, secretion—exhibit themselves as the results of chemical affinity.

And the idea of the animal body, the fundamental conception or plan on which it has been framed, appears to be simply that on which we ourselves act when we wish to construct a machine. We use one modification of force as a resistance to another, privileged herein with the power of imitating, at an infinite distance indeed, the sublimest of the material works of the great Creator of all things.

And further still, this view of life opens to us yet another indication of the unity of principle that binds creation into one. On earth we see the antagonism of two forms of force yielding a well-nigh boundless variety of beautiful, useful, and happy action in the successive grades of animal existence. In the heavens, the antagonism of two forms of force develops the regular motions of the planets, and constitutes the law which ordinates the universe.*

Art. III.

Algeria: its Climate and Merits as a Resort for the Invalid. By Arthur Mitchell, A.M., M.D.

(*Concluded from No. 33, p. 226.*)

"As every country possesses its characteristic vegetable and its characteristic animal kingdoms, so also it possesses its pathological, it has its diseases peculiar to itself, and enjoys an immunity from others."† Or, in the words of our quaint Sir Thomas Browne, "Death hath not only particular stars in heaven, but malevolent places on earth, which single out our infirmities, and strike at our weaker parts."

It is not my intention, however, to enter into the investigation of the whole *pathogenic* features of the climate and soil of Algeria. To one class of diseases my attention has been especially directed—those of the respiratory organs. And it is my present object to adduce and analyse the testimony which supports the belief that pulmonary phthisis is rare in North Africa. Nor is this belief of recent origin, for according to Brunache‡ the consumptive patients of Celsus were sent by him to Egypt and the Mediterranean shores of Africa. Only from the date of the French occupation, however, have statistics been brought to its support.

* I have perhaps failed to indicate with sufficient clearness that the production of functional action by chemical change depends upon the mode in which such change takes place. It is not every decomposition in the living body that necessarily results in a function, but such changes only, and changes of such intensity, as are adapted to act upon the functional mechanism. In a steam-engine it is not every possible expansion of the steam that causes a revolution of the wheels, but only an expansion which takes place in a sufficient and yet limited degree, and in a special direction. In the animal, passive decay of the tissues, as of an unused muscle, and excessive decay, as in some forms of disease, do not cause, but abolish, function.

† Boudin · Traité des Fièvre Intermit., p. 69.

‡ Géog. Méd., par Boudin, p. 21. 1846.

The medical men of the army seem to have been early impressed with it, for nearly twenty years ago (in 1836, only six years after the invasion of the French) the subject was brought prominently before the Academy of Medicine of Paris by Dr. Costellat, who proposed to found an establishment at Algiers for phthisical patients.* On the ground of the statistical element being deficient, the discussion of the Academy terminated with this vote: "It is doubtful if the climate of Algiers can favour the cure of consumption."

This discussion, and the announcement by M. Boudin, in 1840, of his theory of antagonism between tubercular disease and marsh fever, directed the attention of all the French physicians who visited Africa still more particularly to the subject. Hence an amount of evidence has been steadily accumulating in the medical journals to which these men communicated their various experiences. Sometimes their opinions are supported by figures, at other times not; but in all cases they have their measure of value.

From these sources, then, and from the more extended works of Armand, Bertherand, Foley, Martin, Boudin, &c., as well as from private sources, and from the registers of the military hospitals, to which access was given me, I have collected the information which I shall bring to bear on the elucidation of the question.

I have embodied almost all the statistical portion of this evidence in the following table (p. 196), which will form the basis of my conclusions.

This table actually embraces the statistics of about 150,000 cases of disease treated, and upwards of 20,000 deaths; but it will be observed that sometimes the number of cases treated is given, with the proportion of phthisis included, without giving the number of deaths; and very frequently the reverse. If the deaths to those treated were always in the same proportion, this table might be said to represent about 380,000 treated and 27,000 dead. But I shall make no such assumption. Each aspect of the question must rest on the evidence, which is complete as regards itself. The basis is wide enough in all cases to admit of this. Nor in reality will the value of the conclusions be thus weakened, though otherwise a seeming strength might be given them by the introduction of large but fictitious figures.

What proportion, then, do deaths from phthisis bear to the deaths from all causes? This query has to be answered from different points of view. We have an indigenous and a foreign population, and the influence of race has to be ascertained. We have a coast district and an interior, and the influence of residence has to be inquired into. Fortunately, the following table supplies materials for such a reply.

And first, over all classes of the population, and without respect to place of residence—civilian and military; European, Arab, and negro; in hospital and in their own houses; on the coast and in the interior—the table shows that† 20,955 deaths from all causes included 759 deaths from phthisis—being about 1 in 27·6, or 3·6 per cent.

Again, over the European civil population of Algiers, in hospital and their own houses, out of 9262 deaths from all causes, we have 441 from phthisis—equal to 1 in 21, or 4·8 per cent.‡

* Médecine des Arabes, p. 521. E. L. Bertherand.

† From all the data in the table except No. 19. ‡ Nos. 1 & 2 of table.

Authority.	No. of cases treated.	No. of cases of pulmonary phthisis included in these.	No. of cases of diseases of the respiratory organs included in these.	No. of deaths from all causes.	No. of deaths from phthisis included in these.	No. of deaths from diseases of the respiratory organs included.
1. Algiers. Civil Hospital. For the people of the town and plains. 11 years: 1837 to 1847. Furnished to me by Dr. Foley, from his private records	—	—	—	4343	241	553
2. Algiers. European population in their own houses. 5 years: 1843 to 1847. Collected by the Government. Furnished to me by Dr. Foley	—	—	—	4819	200	733
3. Algiers. Mussulman population. 5 years: 1843 to 1847. Collected by Government. Furnished to me by Dr. Foley	—	—	—	3666	130	515
4. Algiers. Mussulman population. 4 years: 1838 to 1841. 'Méd. des Arabes,' p. 525. E. L. Bertherand	—	—	—	3177	78	—
5. Algiers. Military Hospital. 1852, 3, 4. Extracted from the Registers by Dr. Mitchell, with the permission of Dr. Bertherand	19,738	—	—	1044	42	81
6. Algiers. Hôp. Salpêtrière. 1845. Dr. C. Broussais. 'Mém. de Méd. Mil.,' tom. v. p. 60	1047	12	—	63	4	—
7. Algiers. Invalided soldiers returned to France: 1852, 3, 4. Extracted from the records of the Hôpital du Dey, by Dr. Mitchell	1513	15	—	—	—	—
8. Bone. Grat. Consult. 1843 to 1852. Dr. Moreau. Obtained through Dr. Bertherand	50,712	72	3030	—	—	—
9. Oran. M. le Dr. Marseilham. 1838, 9. 'Mém. de Méd. Mil.,' tom. v. p. 52	5578	—	778	541	—	74
10. Orleansville. Civil and Military Hospital. 1852. Dr. Barby. 'Mém. de Méd. Mil.,' tom. xii. p. 149	—	—	—	1376	22	—
11. Tlemcen. M. le Dr. Catteloup. 1842 to 1853. 'Mém. de Méd. Mil.,' tom. xii. 187	12,851	16	—	1008	12	—
12. Tlemcen. 1842. Dr. Cambay. 'Mém. de Méd. Mil.,' tom. lvii.	2698	17	—	108	3	—
13. M. le Dr Gódelier. 'Mém. de Méd. Mil.,' tom. lix. p. 37.	40,341	62	—	—	—	—
14. Garrison of Sidibel-Abbés. Dr. Froissart. 1843, 4, 5, 6. 'Mém. de Méd. Mil.,' tom. lxiii. p. 103	—	—	—	229	2	—
15. Blidah. Dr. Finot. Civil and Military Hospital. 1840, 1, 2. 'Mém. de Méd. Mil.,' tom. lvi. p. 119 ...	9878	5	—	798	10	—
16. Blidah. Civil and Military Hospital. Dr. Laveran. 1840. 'Mém. de Méd Mil.,' tom. lii.	1465	9	—	110	7	—
17. Blidah. June to Dec. 1851. Extracted from Hospital Registers by Dr. Mitchell, given to him by Dr. Laveran	933	4	44	44	1	6
18. Medeah. M. le Dr. Rietschell. 1841. 'Mém. de Méd. Mil.,' tom. lv.	777	—	33	33	—	—
19. Milianah. Dr. Bruguière. 1841. 'Mém. de Méd. Mil.,' tom. lvi.	807	9	—	—	—	—
20. Constantine. Civil and Military Hospital. Nov. 1839 to June 1840. Dr. Deleau. 'Mém. de Méd. Mil.,' tom. lii. p. 236	107	—	—	8	2	—
21. Constantine. European population. Dr. Deleau. 'Mém. de Méd. Mil.,' tom. lii. p. 236. 1838, 39, 40	—	—	—	39	5	—

And of the Mussulman population of Algiers, 6843 deaths embraced 208 from phthisis—giving 1 in 32·9, or 3 per cent.*

* Nos. 3 & 4 of table.

While of the military population in hospital at Algiers, 46 deaths from consumption correspond to 1107 from all causes—having the proportion of 1 in 24·1, or 4·1 per cent.*

Turning now to the influence of locality, we have, out of 17,112 deaths from all causes on the shores of the Mediterranean, 695 arising from pulmonary phthisis—or 1 in 24·6, or 4 per cent.†

While from the statistics of the interior, which unfortunately do not reach a high figure, although they embrace Tlemcen, Blidah, Orleansville, Medeah, Milianah, Constantine, &c., out of 3843 deaths from all causes, we have only 64 from phthisis, or 1 in 60.‡

As regards, then, the relation of deaths from this to deaths from all other diseases, when we compare Algeria with Great Britain and France, we are left in no doubt or difficulty about the conclusion. The line of difference is wide and apparent. Phthisis is the scourge of the latter countries, and but an exceptional visitor in the former.

Of course the value of the conclusion is to be estimated by the value attached to the data on which it rests. When these are still more extended, the relation may be changed a little in either way. I say *a little*, because I think the basis already so broad as to permit no inference that is glaringly erroneous to spring from it. The statistics stretch over seventeen years, and embrace various localities and various races; they were taken from hospital and house practice; they were collected by various observers, and seldom, if ever, with any direct bearing on this question; and they are not an aggregate of those in favour of a particular view, but comprise every figure which I could find published either in serial or monograph literature, or which I could obtain through personal friendship from the hospital registers or the records of private practitioners. The same inference springing from such extensive and diversified materials, however analysed, whether made to speak in parts or as a whole, whether applied to one class or another, to one district or another, must have considerable value.

Let me, however, before further discussing the question, answer the inquiry, what proportion of those who present themselves for treatment in out-door practice, or are admitted into hospital, is made up of phthisical patients?

Out of 123,022 persons treated, 221 cases of phthisis presented themselves, or 1 in 552;§ and out of 49,494 persons treated there were 81 deaths from phthisis, or 1 in 611.||

This appears to give increased force to the conclusion, even after allowing for errors of diagnosis which are more likely to enter into this than into the statement of deaths where the general symptoms must have been clear enough without necessitating much skill in physical diagnosis.

It would appear that this immunity is enjoyed in a higher degree by the Mussulman than the European population; and of the latter, by the military in a higher degree than the civil portion. And again, that those who live in the towns of the interior are still further removed from the chances of death by this disease than those who live on the coast. I was not prepared for this last conclusion. Indeed, I had anticipated the

* Nos. 5 & 6 of table. † Nos. 1 to 6. ‡ Nos. 10 to 21.
§ Nos. 6 to 8, 11 to 13, and 15 to 19. || Nos. 5, 6, 11, 12, 15 to 18, and 20.

reverse, and I think I derived my impression from the medical men whom I met in Algiers—the general opinion being that Bone offers the greatest immunity, after it, Algiers, and least of all, Constantine.* Of all the deductions this rests upon the narrowest basis, but it must remain where it is till further evidence proves or disproves it.

The proportions at which I have arrived of course differ from those found by individual observers, because I have drawn them as far as possible from the sum of all their observations.

Thus Dr. C. Broussais, in a memoir communicated to the Academy of Medicine, gives the proportions as 1 phthisical out of 650 treated, and out of 102 dead.† His documents comprised a mass of 40,000 sick, and I have involved them in the general table.‡

The same author observes that "this disease without any doubt is much less frequent in our African possessions than in France, and the difference is so great that it evidently depends on the climate—no other secondary circumstances being capable of producing it." And he further states his belief that its progress is less rapid, and the chances of cure greater, in Algeria than in France.§

Again, Messrs. Bonafond and Guyon, from the statistics of six years of the mortality of the civil population, give 1 death from phthisis to every 40 from mixed diseases.

And again, according to M. Catteloup, the phthisics are to the entries as 1 to 803, and to the deaths as 1 to 84.‖

While at Bone, M. Moreau found 1 death from phthisis in 42, and his letter to the Academy of Medicine terminates with the following conclusions:—1st. That phthisis is extremely rare among the inhabitants of the country; 2nd. That Europeans are rarely attacked by it; 3rd. That the progress of the disease is arrested in Europeans already the subject of it; and 4th. That it is far from being there a constantly fatal disease.¶

Strong as these are, the conclusions at which M. Odrultz arrives surpass them. Among others he gives the following:**—1st. The climate of Algiers is opposed to the generation as well as to the evolution of tubercle in the lungs; 2nd. This morbid production is observed but very exceptionally among the indigenous population; 3rd. Europeans who do not bring the germ of the disease to Algiers almost never become phthisical; 4th. Those who do bring not only a predisposition, but actually crude tubercle, in greater or less quantity, in the lung, are often cured; or, in the worst case, the progress is extremely slow; 5th. When the tubercle is softened, the climate is no longer favourable.

The averages of all these observers are above mine; but, on the other hand, M. Antonini thinks that about 1 in 20 of the deaths of the military population is caused by phthisis.††

This proportion is below that which results from the data of the table, but I shall not be surprised if eventually this proves to be a very correct appreciation of the proportion of deaths from phthisis over the whole population.

* Martin: Manual d'Hygiène, p. 169. † Boudin: Géog. Méd., p. 25.
‡ No. 13 of table. § Mém. de Méd. Mil., tom. ix. p. 124. ‖ Ibid., tom. xii. p. 187.
¶ Boudin: Géog. Méd., p. 26. ** Annuaire Thérapeutique de M. Bouchardat. 1850.
†† Martin: Manual d'Hygiène, p. 167.

Dr. Martin, after telling us that he believes the medical men of Africa are unanimous about the disease being rare and altogether exceptional among the indigenous population,* gives as his own opinion that it is rare also among Europeans, with whom it "progresses so slowly as to give nature time to organize her means of defence, and consequently of cure. But, moreover, in modifying the constitution it causes it to lose the tuberculous tendency." And, in fine, nothing is more rare among Europeans than tuberculosis developed in the country.†

Drs. Armand and Laveran speak more guardedly on the subject, but the former says, "that no one can contest that among the soldiers, phthisis is less frequent in Algeria than in France;"‡ and the latter, who is an able stethoscopist, furnishes statistics proving the same.

Writing of the same subject, Boudin says:

"The rarity of diseases of the chest is such in Algiers that it has frequently happened to me to visit many hundred sick without having once occasion to apply the stethoscope. Out of a total of 12,853 sick whom I treated in the army of Africa, or in the Lazaret of Marseilles, I only encountered 31 consumptives; and of these 25 had, beyond doubt, been tuberculous before having left for the Morea or Algeria."§

I have still the testimony of another medical man to offer, that of Dr. Bertherand—the present head of the staff at the military hospital of Algiers. He communicated this in writing to me, and I shall give it in his own words:

"I have already had the opportunity," he says, "in several private conversations, of expressing to Dr. Mitchell my opinion of the happy influence which the climate of Algiers exercises both on the development and progress of pulmonary affections.

"A sojourn of more than five years in the military hospitals, in the camps, in European and indigenous towns, in Algiers, Blidah, Constantine, Setif, &c., originated and has every day strengthened the conviction—1st. That phthisis is a disease rare in Algeria, 2nd. That the climate of Algiers arrests, or at least manifestly retards, the progress of commencing tuberculisation; 3rd. That the season of great heat hurries on the work of destruction of advanced tuberculisation.

"I leave to the general statistics with which Dr. Mitchell occupies himself at present with so scrupulous a perseverance, the rigorous demonstration of my first two propositions.

"My intention in this note is to detail more minutely the particular facts of my own private practice.

"From my notes and records of cases, I find that the total number of organic pulmonary affections which I have had occasion to observe since landing in Africa, does not surpass 15.

"Of these, 10 were men and 5 women; 11 were adults, 2 below puberty, and 2 above fifty years of age; 12 were affections existing before arrival in Algeria; 5 are dead, and 10 are still living; 2 died after returning to France in November, 2 in Algiers in May, and 1 in Algiers of cholera in March. Of the 10 alive, 1 has been fifteen years in Africa, 2 have been thirteen, 6 between four and seven years, and 1 for a single year."

I cannot conclude this *résumé* of opinions on this question without citing that of my friend, Dr. Foley, the author of a statistical work 'On the Colonization of Algeria,' and the head of the medical staff of the Civil Hospital of Algiers. His experience has been lengthened and

* Martin: op. cit., p. 164. † Ibid., p. 171.
‡ Armand: L'Algérie Médicale, p. 376. § Traité des Fièvre Intermit., chap. vi.

extensive, and his habits of observation are careful and minute. He frequently communicated to me in conversation his views on this subject, and they always clearly and decidedly went to say that this disease is exceedingly rare in Algiers, both among the European and indigenous population, and that when it is brought to the country, its progress is arrested, and ameliorations of a most marked character take place.

It will be observed that he furnished me with several most important statistical facts. These he extracted from valuable records in reference to the general pathology of the country, which with great labour he has collected, and which I trust he will soon publish.

Negroes in Algeria, to whom this is a northern latitude, fall frequently victims to this disease.* That they do so in Europe is a notorious fact. According to Brunache, in every autopsy he made at Marseilles on a Negro, he found tubercle. Clot-Bey tells us that in Egypt the same thing occurs. At Algiers, Dr. Martin says, "one rarely meets a case of phthisis, except among the negroes;" and Dr. Foley frequently attested the same in conversation.

The negro dies of consumption more and more frequently as you draw him from his own country. Thus, of the negro soldiers at Sierra Leone, their own country, 1000 men gave 6·3 deaths annually from phthisis, while at Gibraltar, 1000 gave 43·; at Honduras, 8·1; at Jamaica, 10·3.† And in America, the mortality from phthisis is twice as great among the negro as among the white population.†

Its exceptional appearance among the Arabs is all but universally admitted. In the foregoing quotations this is frequently expressed, and the table is strongly affirmative.

In addition to this, Dr. Grellois, writing in 1846, says "that he has not seen a single case of phthisis among the Arabs, nor has a single case figured in the Hospital of Ghelma since the 1st of January, 1844."‡

In an old Arabic work on medicine, which M. Pharaon and Dr. Bertherand are at present translating, they tell me that no disease having cough for a symptom is even mentioned.

The Grand Mufti of Algiers frequently told me that the disease was almost unknown among his countrymen.

That it does exist, however, statistics show. Both Bertherand and Armand notice it in their works. The Arabs call it *Meurdh dhaf*,§ or "the disease of weakness," and they believe it to be contagious. The syphilitic cachexy, so prevalent, may be a predisposing cause.|| The actual cautery on the thorax, irritating plasters, Moorish baths, baths of hot sand, exposure to resinous vapours, vegetable infusions with honey, &c., are the remedies most in use among them.¶

Armand** gives an interesting conversation which he held with an Arab doctor, leading to the inference that the disease is *very* common. But this is unmistakeably an exaggeration on the part of the worthy Tebib, and Armand is himself evidently of this opinion—stating that among the Arabs he has much more frequently met with hepatic than pulmonary phthisis. Condensing the colloquy, it runs somewhat thus:

* Méd. des Arabes, p. 524. † Boudin: Path. Comp. pp. 19, 20.
‡ Mém. de Méd. Mil., vol. lx. § Bertherand: Méd. des Arabes, p. 523.
|| Ibid. ¶ Ibid. ** L'Algérie Médicale.

Speaking of diseases of the chest—"Thou knowest," said Ben Chaoua, "that they are common here."

"Thou knowest," said Armand, in turn, "that there are *colds* in the season of the rain, which are not serious diseases."

"Yes," was the reply, "but these colds, without being grave, are in summer sometimes very obstinate."

"It is very true." [He spoke, no doubt, of bronchitis.]

"But there is another disease of the chest," continued Armand, "where the patient cannot cough, or at any rate the cough is small and dry, and very painful in consequence of a stitch in the side."

"There," was the rejoinder, "you have a more serious disease, with fever—I know it."

"There is still another," said Armand, "where there is great fever, no stitch, cough, great oppression, and expectoration mixed with blood."

"I know the disease," replied Ben Chaoua; "when it is not well cured, the patient dies in the long run, always coughing."

"In fine," said Armand, "there is yet another chest disease, which shows itself especially in the young and feeble, the pale, the ill-fed, ill-lodged, and ill-clothed, and more certainly if their parents have had the same disease. They begin by coughing a little, then more, sometimes with blood in the expectoration, fever takes place, and night-sweats, the spit becomes thick and puriform, and sometimes comes up as if vomited, and they die off by a decline, giving up the last breath suddenly, sometimes in speaking, sometimes in eating."

"Oh! this disease is very common among the Arabs," said the Tebib.

"Very common?"

"Yes, more so than fever or dysentery."

After having satisfied himself that M. Jacob, the interpreter, had faithfully done his task, and that Ben Chaoua was not talking of some other ailment, M. Armand asked him how he treated such cases.

"Oh! you French medical men torment the sick by bleeding, blistering, and plastering, and you leave them to die of hunger."

"Very much obliged," interpolated Armand.

"The empirical Arabs do just the reverse."

"And what may that be?"

"When a young man," said Chaoua, "perceives that he has this disease, he gives up every occupation; eats everything he fancies, using strong spices and drinking strong liquors; he takes exercise on foot and on horseback; he gives himself up to women;" &c.

"That is to say, he follows a life which would kill a robust man. He ought to finish his existence soon by such a course."

"There are those who are cured by it," was the sententious reply.

"You think so?" queried Armand.

"I am certain of it."

The conversation is more curious than valuable. It does not give the opinion of a Rhazes or an Avicenna, but the little known Ben Chaoua. And an uneducated Tebib and an unprofessional interpreter are bad elements. Nevertheless, it will be observed with interest that the principle of the "sustaining treatment" of the present day—the cod-liver oil, beef-steaks, porter, and open air—exists there, although abused, it is true.

We are naturally led here to the comparison of the mortality from phthisis in Algeria with that in other countries, and it cannot be done more briefly or clearly than in a tabular form.

The Mortality from Phthisis compared with the Mortality from all Causes in different localities.

		Proportion.	
		Deaths from phthisis.	Deaths from all causes.
Algeria	All classes of population	1	27·6
	Europeans—civil	1	21·0
	Europeans—military	1	24·1
	Mussulmans	1	32·9
London*		1	8·1
England and Wales†		1	5·3
Paris‡		1	5·0
French Army§		1	5·0
Marseilles‖		1	4·0
Genoa¶		1	6·9
Nice**		1	7·0
Naples††		1	8·0
Gibraltar, Malta, and the Ionian Isles‡‡		1	3·8
New York§§		1	7·2
Boston§§		1	6·6
Baltimore§§		1	5·4
Charleston§§		1	6·9

Phthisis, then, must become greatly more prevalent in Algeria before it stands on a parallel with Europe or America.

It may be alleged that, in consequence of the great prevalence of other diseases, it is possible that tubercle may exist in the lungs of the many who fall under fever or dysentery, but that they are cut off before it manifests itself or calls attention.

Many facts in my possession go to the removal of this objection.

Out of 1104 autopsies performed at Tlemcen by Dr. Catteloup, in which tubercle was sought for in the lungs, it was found in 88, being in the proportion of 1 to 13. These had died of the following diseases:

Dysentery	71
Marsh cachexy	4
Chronic bronchitis	2
Hepatic affection	2
Cholera	5
Different diseases	4
Total	88

Again, M. Broussais mentions that only 3 cases of tubercle were found in the autopsies of all those who had died of acute disease or of any other disease, except phthisis; while in Paris, and in France generally, under like circumstances, he says that the occurrence is frequent.

* Col. Sykes: Statistics of Nice, p. 36. † Craigie: Practice of Physic, p. 1000.
‡ Broussais: Mém. de Méd. Mil., tom. lx. 124; and Andral and Boudet.
§ Benoiston de Chateauneuf. ‖ Boudin: Géog. Méd., tom. xxxiv.
¶ Sykes: op cit., p. 36. ** Andral: op. cit.
†† Andral: Traité de Path. Intern., tom. i. p. 449.
‡‡ Godelier: Mém. de Méd. Mil., vol. lix. §§ Swett on Diseases of the Chest.

And Dr. Leclerc, who was stationed at Cherchell for six years as *chef de l'hôpital*, stated to me that he had while there some 500 or 600 necropsies; and among these the presence of any tuberculous deposit in the lungs was not established in above 5 or 6, or about 1 per cent. From particular circumstances, he said, the inmates of this hospital were entirely composed of those who had been resident in Africa for a considerable time, and in whose cases, therefore, the climate had had time enough to manifest its influences. In France, with a similar medical charge—that is, with the same class of men—he said that he found tubercle in nearly one-third of the autopsies:

"I cannot but believe," he added, "that many of these men arrived in Africa with latent or nascent tubercle in the lung, although apparently, perhaps, in good health: from which it would appear, that not only does the climate of Africa prevent the development or arrest the progress of the disease, but that there ensues a more radical change—the actual disappearance or removal of the deposit."

It is worthy of remark, that Dr. Leclerc himself labours under chest affection, and has now selected Algiers as his residence, from his high opinion of the climate.

The reasoning of Dr. Leclerc must be correct, if it be true, as asserts M. Boudet, that five out of seven of the population of Paris have tubercle in some form and to some extent in their lungs.

M. Boudin tells us that five persons returned to France on account of dysentery, who, before their sojourn in Algeria, had presented the characteristic symptoms of phthisis; and who, on their return, appeared to him to be entirely cured of this affection. Two of these five died eventually of disease of the large bowel; and the necropsy confirmed the absence of tubercle in the lung as well as in other organs.*

Brunache cites three instances of persons who, with decided symptoms of tubercle in the lung, went to Algiers, and enjoyed good health while there; but in whom, on their return to France, chest symptoms reappeared, and death ultimately ensued from consumption.

Another objection which may be brought against the validity of the statistics is this—that, among the military at least, a large proportion of those who are sent home invalided consist of phthisical patients.

To remove this, I examined the hospital registers for three years (1852-3-4), and with the following result:—Out of 1513 who obtained *congés*, there were 15 cases of consumption; and the 1499 consisted of those suffering from the effects of fevers, dysentery, accidents, &c., &c. This gives just one phthisical per cent. of the invalids returned to France.

It is possible, however, that the whole mortality of Algeria is so high, that although phthisis may bear a small proportion to the whole number of deaths, it may still bear heavily on the effective of the population.

There is no doubt that the rate of mortality over the whole population of Algeria is higher than on the continent of Europe or in Great Britain. It is difficult, if not impossible, however, to give the exact proportion, as it has been so variously estimated by the friends and adversaries of colonization. Statistics of course exist, and the result I shall now give.

The general mortality of Europeans in Algiers, from the 'Tableaux de

* Boudin: Géog. Méd., p. 39.

la Situation des Établissement Français' (official documents), has given an average since the conquest of about forty-four to forty-five deaths out of 1000 individuals.* According to Martin and Foley, many circumstances make this a high statement: amongst others, the including of the still-born, who constitute in Algiers one out of every eleven births.

For the town of Algiers itself, from the same official documents, an average of eighteen years gives 43·5 out of 1000.† But the deaths in Algiers represent a larger population than that of the town, since its large hospitals receive the sick from the villages in the adjoining plains. If this and other sources of error be deducted, according to Martin and Foley, it is reduced from 45 to 31·5 out of 1000 of the population.‡

Among the military population, 1848 showed the deaths to the effective as 36 to 1000. But here again, as colonists and natives were admitted in considerable numbers into the hospitals which furnish the statistics for the above, and ought therefore to be deducted, according to the same observers the mortality will fall from 36 to 24.

I am well aware that M. Boudin, M. Desjobert, and the opponents of colonization in Algeria, keep it at the figure I first stated; but from a perusal of the works on both sides, I feel inclined to think that they somewhat over-estimate it, and that the smaller proportion is nearest to the truth.

Comparing, then, the general mortality here with that in Europe, we have—

For Algeria, European civil population	31·5 (or 45) in 1000	
,, Ditto European military population	24·0 (or 36) in 1000	
,, France, civil§	24·0	,,
,, Ditto, military§	19·0	,,
,, Prussia‖	30·6	,,

There is clearly, therefore, no such excess of the general mortality as to account for the small proportion which deaths from phthisis bear to the deaths from all causes.

Diseases of the Respiratory Organs generally.

Though phthisis is rare, it does not necessarily follow that other diseases of the respiratory organs are also rare. This is another and an important aspect of the question. The original table leads to the solution of this also.

I. Of the European population of Algiers in hospital and in their own houses, 9262 deaths include 1286 from diseases of the respiratory organs, or 1 in 7·1, or 14 per cent.¶

II. Again, of 57,223 cases treated in hospital, dispensing, and private practice, 3852 laboured under some form or other of chest affection, which gives the proportion of 1 in 15·1, or 6·6 per cent.**

III. And again, 26,249 cases treated, gave 161 deaths from disease of the respiratory organs, or 1 in 163.††

* Foley et Martin: Histoire Statistique de la Colonisation, p. 323. † Ibid. ‡ Ibid.
§ Desjobert: Documents Stat. sur l'Algérie, p. 2.
‖ Stat. de la Pop. de l'Europe, p. 61. ¶ Nos. 1 & 2 of table.
** Nos. 8 & 9 of table. †† Nos. 5, 9, & 17 of table.

Before commenting on these results, let us ascertain what particular diseases of the respiratory organs compose these numbers.

And, first, of those who died of miscellaneous diseases of the chest, we find that out of a total of 1882,*

Phthisis	gave	613
Chronic and acute pneumonia	„	431
Chronic and acute bronchitis	„	396
Pleuritis	„	144
Pleuro-pneumonia	„	55
Empyema, emphysema, and hæmoptysis	„	5
Thoracic affections not named	„	238
Total		1882

This total corresponds to 13,872 deaths from all causes.

Secondly, of those who were treated for miscellaneous diseases of the respiratory organs, an aggregate of 3030 is divided thus:†

Phthisis	72
Pneumonia	61
Bronchitis	1987
Pleuritis	359
Croup and laryngeal disease	20
Hooping cough	235
Influenza	62
Hæmoptysis, angina, asthma, asphyxia	234
	3030

Corresponding to 50,712 treated for all diseases.

Are these proportions greater or smaller than in Europe? They are much smaller, as will be seen from the subjoined table.

Proportion of Deaths from Diseases of the Respiratory Organs to Deaths from all causes in different places.

In Algeria	4·0	per cent.
‡In Paris	33·0	„
§In London	31·5	„
§In Nice	25·1	„
§In Genoa	31·0	„
§In Turin	38·2	„

And according to the statistics of M. Trébuchet, one-third of the whole mortality in Paris is made up from the three diseases—phthisis, pneumonia, and bronchitis (catarrh). Thus from ten years' observations,

Phthisis gave an average of	4261	deaths annually.
Catarrh „ „	2222	„
Pneumonia „ „	2634	„
	9117	

Or one-third of the whole average mortality.‖

* Nos. 1, 2, 3, 5, & 17. † No. 8.

‡ Trébuchet: Annales d'Hygiène, vol. xlvi. § Sykes: op cit.

‖ Trébuchet: Annales d'Hygiène, vol. xlvi.

A remark of Dr. Martin, in reference to the progress of one of these diseases, is worthy of notice. He says that he has never seen a case of pneumonia originating under his own observation in Algeria, pass into phthisis; and he concludes from this, that there is some antipathy between the climate of Algiers and the generation of tubercle.*

Influence of Season on Deaths from Phthisis in Algeria.

The influence of season on the mortality by phthisis is not without interest, and to some extent I am able to indicate it. Six hundred and thirty-six deaths by this disease were thus divided over the year:

January	65	145	July	51	164
February	33		August	57	
March	47		September	56	
April	56	160	October	61	167
May	53		November	55	
June	51		December	51	

The general impression, I think, is, that August, September, and October are the months most fatal to phthisical patients; but the above table does not point to any month or months as being especially fatal, although the first three months of the year are certainly, on the whole, the least fatal.

In France it is otherwise. According to M. Trébuchet's statistics for Paris, between 1839 and 1849, March, April, and May are decidedly the most fatal, and September, October, and November the least so.†

The general mortality in Algiers, however, is greatly diminished during the first three months of the year; and twice as many are treated for all classes of disease during the last half of the year as during the first (83,379 and 39,913) ‡

As far as *surgical practice bears upon this question*, I learned from Dr. Bertherand, that almost no surgical case presents itself arising from a scrofulous habit of body. "*White swelling*," he said, "I may say, never appears amongst the soldiers in Africa—certainly not one-tenth of the number of such cases we meet in similar hospitals in France." And to the note giving me his experience of phthisis, he appends the following: "Wounds of the chest by fire-arms are recovered from in Africa in a proportion truly surprising. The cause has been too exclusively assigned, I think, to the smallness of the balls used by the Arabs; and it has been too much lost sight of, that the gravity of these wounds does not altogether depend on the injury, greater or less in extent, which the projectile produces in the lung, but more especially on the splinters of bone, &c., which it takes with it. Be this as it may, however, the fact itself cannot be denied." He then details numerous instances of recovery under his own care, and concludes with this query: "May not a part of these happy terminations be, with justice, attributed to this—that the injured portion of the parenchyma is acted on here by an atmosphere less irritating than in other latitudes; and by inference, may we not draw from

* Martin: op. cit., p. 171. † Annal. d'Hygiène, vol xlvi.
‡ Finot: Mém. de Méd. Mil., vol. lvi.

the fact, some favourable considerations in the question of the organic pathology of the lung?"

I have throughout the whole of the discussion of this subject rather stated the opinions of others than delivered any of my own. It has been my wish simply to expose the evidence, and I have endeavoured to do this as fully as possible.

I have offered no probable explanations. It may be a question of climate or of soil, or of both, or of neither. With this I have not dealt. My object was to ascertain on what the belief rested, to what extent it existed, and how far it was supported by statistics.

It would appear, then, that although on some points of the pathology of Algiers, difference of opinion may exist among the medical men who have had to do with disease there, on one they are all agreed,—and that is the one we have discussed. Some hold it more moderately than others, but all believe in the truth of the general statement, and if *quod ab omnibus creditum est, verum est,* we must receive it too.

My own summing of the evidence would be this:—

1. That the foregoing figures and expressed medical opinions authorize us in concluding that phthisis is a disease considerably rarer in Algeria than in Europe or North America.

2. That on the same testimony we are also warranted in concluding that other diseases of the respiratory organs are less frequent in Algeria.

3. That from the extent and character of the evidence adduced, it is probable that these general inferences will stand the test of future inquiry.

I confine myself to these, and therefore within narrower bounds than the French physicians have usually done. Yet it is true that there are certain points of this evidence which support the view that in Algiers the development of tubercle is prevented to some extent in those predisposed to it, and that in those in whom it already exists—while in an early stage—the progress of the disease is arrested, and such a complete disappearance of the general symptoms induced, as to be called *a cure.* Such a conclusion is much more uncertain than are those I have drawn, which must be considered the only legitimate ones. But though it does rest on a less satisfactory basis, one would almost wish to believe that it contained a measure of truth, and that thus the unfortunate who seeks Algiers labouring under that ailment, "wherein," as Sir Thomas Browne hath it, "with us it is as dangerous to be sentenced by his physician as by a judge," may not "fruitlessly be put in hope of advantage by change of climate." We may add with the same author: "He is happily seated who lives in places whose air, earth, and water promote not the infirmities of his weaker parts, or is early removed into regions that correct them."

Art. IV.

Clinical Observations on Hare-lip. By Hermann Friedberg, M.D., Lecturer on Surgery and Forensic Medicine in the University of Berlin.

In the ensuing pages I propose presenting to the reader a few cases of hare-lip which occurred in my surgical wards, and offered circumstances of peculiar interest. I shall first relate a few cases in detail, and then only advert to the leading features of others, in order to illustrate the proceedings which I have adopted in operating for this malformation, and also for the purpose of weaving in or adding some physiological and surgical remarks. Thus, to the first case I have appended observations on the treatment of the inter-maxillary bone in operating for hare-lip, and on the spontaneous cure of fissured palate after this operation. After Case 2, I have examined the relation which the hare-lip bore to the co-existing malformation of the basis. I have taken the opportunity of at the same time investigating the abnormal state of the eyes presented in this case. The formation of the red margin of the lip has also appeared to me to merit a careful consideration. In going over this ground, I have first had regard to the ordinary method of operation, and have then adverted to that adopted by Pétrequin, Mirault, and Malgaigne, for the purpose of obviating the angular inflection of the red margin of the lips, which is so apt to result from the ordinary proceeding. I have next illustrated by several cases my own mode of procedure, which consists in forming an arched projection with a broad base under the point of junction of the lateral parts of the lips, and to form the edge of the lip by transplanting the red margin. Lastly, I have given a summary of the principles which should guide us in the choice of a method for the formation of a red border of the lips.

Case I.—Hare-lip on the right side, involving the palate; absence of the right half of the upper lip; the nose disfigured by the lower half being pushed over to the left side. Operation for the lip performed fourteen hours after birth. Repair of the right half of the upper lip by transposition of a flap excised out of the cheek; the margin of the lip formed by making an arched button or projection with a broad basis, below the point of juncture of the lateral parts of the lip; detachment of the inferior half of the nose from its insertion, for the purpose of straightening it; cure; gradual retrocession of the inter-maxillary bone, and spontaneous closure of the palatal fissure.

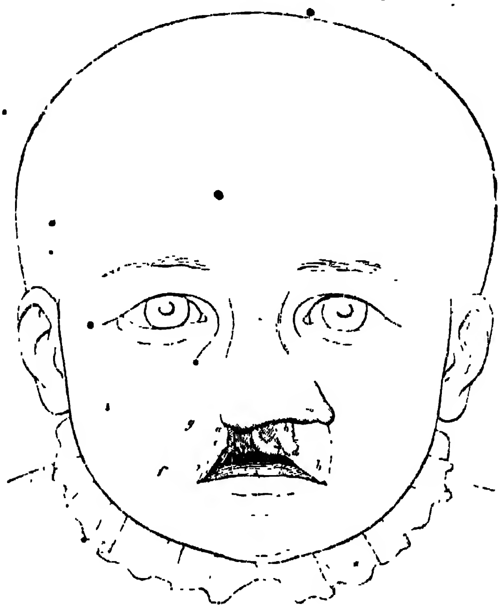
Heinrich W——, the third child of healthy parents, was born with a hare lip. His mother had enjoyed perfect health during her pregnancy; the two older children were born without malformation. Twelve hours after birth, on the 7th March, 1853, the child was brought to my clinique, and presented the appearance represented in fig. A.* (p. 509.)

The boy was plump, and well-formed, with the exception of the anomaly to be shortly mentioned. The upper part of the nose was

* The original drawings of the woodcuts have been executed by the well-known artist, Mr. Andorff, of Berlin. The dotted lines in fig. A indicate the incisions to be made for the operation of hare-lip.

rather narrow; the inferior portion was remarkably broad, and malformed; the tip of the nose was turned upwards, and pushed over to the

Fig. A.



left side; the left nostril appeared contracted, the left ala pushed upwards. The septum nasi rested in front upon the inter-maxillary bone, and posteriorly on the left margin of the palatal fissure shortly to be mentioned. The right ala nasi was remarkably drawn over to the right; its point of insertion passed imperceptibly into the right cheek, and lay close to the upper end of the right edge of the labial fissure. The right supra-maxillary portion of the lip was entirely deficient, so that no trace of the sulcus naso labialis was perceptible on this side, whilst it was distinctly visible on the left side. The only indication of the upper lip on the right side consisted in a narrow red labial streak, which extended from the angle of the mouth towards the external termination of the right ala nasi, was lost about three lines below it, and formed a border to the edge of the right cheek, which was turned towards the labial fissure. The labial fissure was very broad, and in the right nostril led into a fissure of the right side of the palate between the inter-maxillary bone and the right superior maxilla. This fissure extended backwards between the maxillary processes of the superior maxillæ. The posterior portion of the palatal fissure lay more to the right than in the middle of the oral vault, and terminated at the horizontal processes of the palatal bone, at a short distance anterior to the velum, which was perfectly normal.

Accordingly, the fissure appeared to depend mainly upon an imperfect development of the right superior maxilla. The left termination of the

to approximate to one another the two edges of the incision into the cheek (fig. A, *f*, *g* and *a*, *g*); they were at once attached to one another by stitches and one figure-of-8 suture (fig. B, *i*, *k*). The red margin of the left portion of the lip (fig. A, *e b*) was next separated from above downwards to the vicinity of the left angle of the mouth, and turned down; the upper part of the left margin of the fissure was merely scarified. The flap from the cheek was now laid into the right half of the space belonging to the upper lip, so that the red margin faced downwards (fig. B, *o*, *m*, *n*); the latter was detached as far as the vicinity of the right angle of the mouth (fig. A, *c*, *r*), and turned down. The superjacent edge of the cicatrix of the right margin of the fissure was removed by an incision (fig. A, *c*, *g*), which also detached the upper angle of the red margin (fig. A, *c*, *g*). The transplantation of the flap from the cheek converted what had been its external edge (fig. A, *f*, *g*) into the upper margin of the right half of the lip and the floor of the right nostril (fig. B, *i*, *l*); what had been the upper edge (fig. A. *a*, *g*), now was directed from the septum narium towards the lower edge of the upper lip, and was united to the right margin of the left portion of the lip (fig. B, *l*, *n*).

By the incisions *a*, *g* and *c*, *d* (fig. A), the two free ends of the dependent red edge of the lips received the following conformation: the upper angle of the left red margin (fig. A, *d*, *e*) projected, while the upper angle of the right red margin (fig. A, *c*, *g*) was slanted off. For this reason the two angles, when their cut surfaces were placed in apposition, fitted well, and could be united by fine stitches. The red margin now represented a continuous arch, with a short fold projecting downwards. This fold I laid under the point of union of the two halves of the lips (fig. B. *n*), then pushed the right and left edges of the red margin of the lip together, so as to form an arched prominence, which was at once attached to the lip. I distributed the remainder of the red margin, and fastened it to the right and left under the lip (fig. B, *e*. *n* and *n*. *p*) in such wise that the arched prominence was less marked (fig. B. *n*, *m*). In order still further to secure the labial halves in apposition, I passed a few narrow strips of court-plaster from one cheek to the other, and covered them with collodion.

The child bore this severe operation well. There was scarcely any hæmorrhage, owing to the external maxillary artery having been compressed during the whole operation by Dr. Wilhelm, who kindly assisted me. The position and form of the nose appeared much improved (fig. B). I proposed to rectify the great width of the right ala nasi subsequently, if it should be necessary, by excising a triangular wedge, of which the base would be turned to the free margin. Soon after the operation, and three times within the next twenty-four hours, the child had attacks of dyspnœa,* which were rapidly removed by warm baths and friction of the thorax with the flat hand. Internally, half a teaspoonful of syrupus croci and syrupus diacodion was administered every half hour, which induced sleep of several hours, during which the infant breathed calmly with an open mouth. On the second day after the operation two sutures were removed, on account of the tension induced; throughout, union took place by first intention, excepting at the point of the right cheek from

* See Chirurgische Klinik, Beobachtungen und Erläuterungen im Gebiete der Chirurgie. Von Dr. Friedberg. Band i. p. 200. 1855.—(On the Nature of the Dyspnœa following the operation for Hare-lip.)

which the right half of the lip was taken; here the edges of the wound (fig. B, *i*, *k*) partly separated, and subsequently united by granulations. But very little tumefaction ensued in the parts, and no peculiar features were presented. From the fourth day after the operation the child took the breast. On the subsequent day I was enabled to remove all the sutures, and replace them by strips of plaster. The floor of the right nostril (fig. B, *i*, *l*) rapidly cicatrised, and the operation could now be termed completely successful.

Two months later I saw the child again, and found it in good condition. The fissure of the hard palate was diminished by one half, the inter-maxillary bone no longer projected, and had united with the alveolar process of the right superior maxilla, with the exception of a minute indentation. In the tenth month after the operation, after the middle lower incisors had made their appearance, the palatal fissure had completely closed. The red margin of the lip exhibited a symmetrical and wavy line on both sides of the arched prominence which occupied the middle; the lip presented an outline closely resembling that of the normal condition.

I avoided the extirpation of the inter-maxillary bone in the case just detailed, although it had an abnormal direction, and projected beyond the level of the labial fissure. I diminished the prominence of the bone by bending it back towards the palatal fissure, so far as its union with the left upper maxilla permitted. In case of this proceeding failing, it was to be feared that the labial union which lay before the projecting part of the inter-maxillary bone might advance a little; but I might equally expect that the pressure exercised by the lip upon the bone would gradually push it back into its proper place. The result has shown that the latter hope was justified. In all the cases of hare-lip which I have hitherto observed, it was possible to push back the projecting inter-maxillary bone by the pressure exerted upon it, with the fingers or by means of a forceps, sufficiently to effect the union of the lips. The branches of the forceps with which the inter-maxillary bone is seized should be covered with leather or linen, so that the mucous membrane may be protected as much as possible.

This proceeding necessarily induces a fracture of the nasal septum. If the inter-maxillary bone is so far detached as to fall down, Mr. Butcher* recommends that it be fastened with wires to the upper maxilla; this procedure could necessarily only succeed if, in detaching the inter-maxillary bone, the vascular connexion remains sufficiently intact to secure its due nutrition.

In the boy whose case I have just detailed, the pressure of the fingers sufficed the more readily to push back the inter-maxillary bone, as the septum narium had previously been divided along the floor of the nares. I have not yet had occasion to adopt the proceeding of M. Blandin, which consists in excising a cuneiform portion of the septum. In no case do I consider extirpation of the inter-maxillary bone to be necessary, solely on account of its too great prominence. If this bone is removed, the alveolar arch of the upper maxilla will eventually prove too small for the lower

* The Dublin Quarterly Journal of Medical Science, p. 43. Feb., 1856.

jaw by four incisors; from the two jaws not fitting, the chin will project much, and mastication will be rendered difficult. If the inter-maxillary bone is distorted, or much retarded in its development, it is better to remove, as was done in Case 2.*

Numerous cases have been published in which, after the hare-lip had been operated upon, a spontaneous closure of the fissure in the palate ensued. It appears that the fissure in all these cases only affected the hard palate. In addition to the case above given, I am able to quote another, a girl, who was operated upon in my clinical wards ten hours after birth, in whom the palatal fissure healed spontaneously. In both cases the fissures ceased anteriorly to the ossa palati. The girl just mentioned had a double hare-lip, involving the palate. The fissure on the right side of the inter-maxillary bone was two lines broad, that on the left three lines broad; between the palatal processes of the superior maxilla it was about two lines, further back about three lines in breadth. At the age of thirteen months, the palatal fissure was closed, with the exception of a portion, one line broad, posteriorly. Six months later, the child was re-admitted, on account of some other complaint, and no trace of the opening remained.

The spontaneous cure of fissured palate is partly the result of the progressive growth of the inter- and supra-maxillary bones; it is partly brought about by the pressure exercised upon the bones, when the operation of hare-lip has been successful, by the upper lip and the cheeks. As this influence is more powerful in front than behind, the closure of the fissure takes place earlier anteriorly than posteriorly. Where this influence is not brought to bear, in consequence of the labial junction not having been effected, the fissure of the palate does not close spontaneously. In two cases of this kind I was able to determine that the fissure had increased in width after birth; in these cases the bones in relation with the fissure must have separated still further, owing to the absence of any retentive medium. The circumstance, that we meet with infants who have a fissured palate, with an entire upper lip, indicates a very feeble development of the bones in relation with the fissure, and does not invalidate the view just alluded to. As further evidence of the pressure exerted upon the subjacent bones by the upper lip, I may advert to the fact that if, during the existence of hare-lip, a tooth breaks through in its vicinity, the latter, instead of descending, advances, and pushes before it the edges of the fissure. Hyrtl* remarked, justly, that the pressure exercised upon the alveolar process of the upper jaw by the upper lip, maintains the teeth as they appear, in due order.†

Another main reason why the palatal fissure earlier closes in front than behind, is to be found in the fact that the inter-maxillary bone and the alveolar process of the superior maxilla are developed more rapidly during the first years of life, than the other bones connected with the palatal fissure.

It has been asserted that the union of the lips in labio-palatal hare-lip

* See his Handbuch der topographischen Anatomie und ihrer praktisch medicinisch chirurgischen Anwendungen. Band i. p. 224.

† He alludes to the fact that when a tooth pierces the anterior wall of the alveolar process, the lip may be perforated, so that the tooth appears on the face.

is of no moment with regard to the nutrition of the child, because the obstacle to suction lay in the palatal fissure. I decidedly demur to this view. Children affected with labio-palatal hare-lip are only then unable to suck when there is a palatal fissure on each side of the inter-maxillary bone (*palatum fissum duplex*), when the fissure is broader than the nipple, or when it is combined with fissured lip. If the lip is closed, and the palatal fissure only narrow and confined to one side, the children are able (like Heinrich W.) to take the breast. They seize the nipple as low as possible, press it against the edges of the palatal fissure, and thus force out the milk.

Considering the injurious influence exercised by the fissure of the palate upon nutrition, and at a later period upon articulation, and considering the influence exerted by the union of the lips upon the contraction of the fissure in the bone, it is the more necessary to form a high estimate of the early operation for hare lip. I cannot agree with Hyrtl* and Dupuytren,† who consider it impolitic to operate soon after birth, as the great mortality among new-born infants might cause the operation to be regarded as the cause of a fatal issue. I have on another occasion combated the fear of operations upon new-born infants.‡

Mr. Butcher§ remarks that the fissure of the palate often closes spontaneously, if the hare-lip is operated upon in infancy. But it does not appear from his treatise, which is so rich in practical suggestions and scientific data, that he has himself observed this circumstance; it is not definitely stated in the eight cases which he has published, that he observed this result. The earliest time at which he operates is the twelfth day after birth; and he is of opinion that the operation ought not to be performed before the end of the first week, in order, as it were, to allow the "functions of the body to be healthily in action." The conclusive grounds upon which he opposes the postponement of the operation, induces us to hope that he may think proper to operate still earlier, since there seems no reason why the functions of the body should be in particular order at the end of the first week.

The question arises as to which functions are involved in the treatment of hare-lip. As soon as the tissues of one cut surface can grow into those of the opposite side, the condition for the union of the cut surfaces is fulfilled. But this condition is realized during fœtal life, as is shown by the cicatrisation of the stump after spontaneous amputation before birth. Dr. West‖ has published a case of this kind, in which the leg of a new-born child had been removed an inch and a half below the knee, and the cicatrising process of the stump was far advanced. Mr. Butcher¶ quotes a case in which the stumps of the toes of a new-born infant showed a recent cicatrix; in another case, the stump of a finger exhibited a perfect cicatrix. The same was observed in the case of an infant, by Chaussier,** where the amputated fore-arm lay by the side of the child. A series of

* Loc. cit. p. 226. † Chirurgie Clinique, tom. iv. p. 90.
‡ Chirurgische Klinik, Band i. p. 199.
§ On the Operative Measures necessary in the Treatment of Hare-lip: Dublin Quarterly Journal of Medical Science, No. xli. p. 30.
‖ London Medical and Surgical Journal, vol. i. p. 741. 1832. ¶ Loc. cit. p. 27.
** Discours prononcé à l'Hospice de la Maternité. 1812.

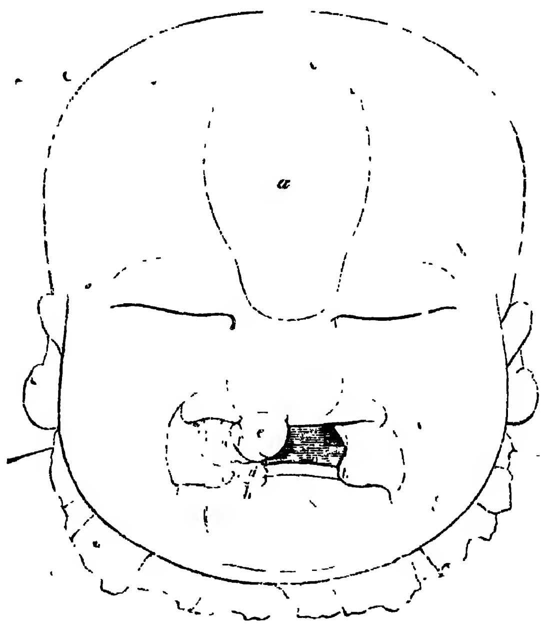
analogous instances has been recorded in Germany. The warmth of the uterine cavity necessarily exerts an influence, but the powers of reproduction are sufficient, even after birth, to secure the healing of wounds, as is shown by the perfect result obtained by operating for hare-lip within a few hours of birth. I have shown, in my 'Clinische Beobachtungen,' that the reaction that follows an injury is the less the sooner the operation is performed after birth.

Case II.—Double labio-palatal fissure, with microphthalmia, resulting from malformation of the brain. Proposed method of operating for hare-lip. Death. Necropsy.

Mrs. L., whose previous children were well formed and healthy, had a fifth child, a boy, who was born with a hare-lip, and presented several deviations from the normal state. It was brought to my clinique when twenty days old. The child had not at that time opened its eyes, had slept much, and had shown a good appetite. It could not suck, but took the milk that was given it with avidity, though a large portion was rejected by the nostrils, with frequent symptoms of urgent dyspnœa. During these attacks the child was raised; it coughed, and soon was pacified. The amount of nourishment thus taken was evidently insufficient, because the infant became more voracious, and the dyspnœa at the same time increased when it took food. A fortnight after birth it had diarrhœa. Two days later, the child was found to have aphthæ, to become restless and fretful. Vomiting supervened, and the milk that was rejected was not at all, or but slightly, coagulated.

On admission, on the 1st July, 1853, the child presented the following appearance:—

Fig. C.



The child was of normal size and well formed, with exception of the head, which was decidedly too small in proportion to the size of the child, especially the cranium. The circumference of the cranium was $11\frac{7}{8}$ inches, the antero-posterior diameter was 4, and the transverse diameter 3 inches. The forehead presented a peculiar malformation, as both ossa frontis formed a pointed projection resembling a cockscomb (fig. c, *a*), the anterior edge of which felt like a sharp ridge of bone. The anterior fontanelle did not present any peculiar extent, but was drawn out towards the frontal suture, to a long point. The occiput was normal in shape.

The eyes were remarkably close to one another; the lids showed no trace of convexity, they were so firmly closed that I could only separate them by means of a speculum; considerable resistance was offered to this instrument by the constrictor palpebrarum. The orbits were much too small; the right eyeball very small, and the left one almost imperceptible.

The nose was of normal length, very broad, and prominent below; the bridge of the nose was very narrow and shallow, and visibly depressed under the crest of the forehead. The left ala nasi was broader than the one on the right. A flap of skin, four to five lines long, and two lines broad (fig. c, *e*), hung down from the tip of the nose, and showed a piece of red labial tissue at its inferior extremity; it corresponded to the cutaneous covering of the septum, the philtrum, and mesial portion of the upper lip. Besides these defects, there was a double labio-palatal hare-lip. To the right of the cutaneous fold just spoken of, the remarkably small and narrow inter-maxillary bone projected so as to advance three lines beyond the level of the two lateral portions of the upper lip; it was turned to the right, and approached close to the tip of the nose. The left external incisor (*h*) had already made its appearance at the anterior much-distorted end of this bone, but was badly developed. At both sides of the inter-maxillary bone the palatal fissure was visible, and posteriorly it formed a broad slit in the hard and soft palate, and divided the uvula into two equal parts. The lateral portions of the upper lip were pretty symmetrically formed, and showed a well-developed red labial margin, which to a small extent inverted the edges of the fissure (fig. c, *b* and *c*). The oral mucous membrane presented numerous catarrhal ulcers, and was of a vivid red. The navel was perfectly healed, and the scrotum contained both testicles.

The crest on the os frontis indicated a fusion of the two halves of the bone, and this, taken with the irregularity in the features, induced me to assume a fusion of the two cerebral hemispheres, closely approaching to cyclopia. The smallness of the skull, and the close approximation of the eyes, also suggested a scanty development of the brain.

The inter-maxillary bone, though atrophic, was present; there were also two orbits, though below the normal size; lastly, the child had a nose of sufficient length, with two nares; consequently, we had not to do with a fusion of the two halves of the face and the mesial portion of the face. The nose and the inter-maxillary bones were present. But when complete fusion of the two halves of the cerebrum is associated with microcephalic malformation, we generally find the two halves of the face also

fused together, and the mesial portion defective. On this ground, I concluded that the assumed fusion of the cerebral hemispheres was incomplete.

Long life was scarcely to be expected, nor desired, for a child presenting such malformations as those adverted to. Still, as such a child has an undoubted right to a prolongation of life, and this was much endangered by the labio-palatal fissure, I considered it to be my duty to perform the operation for hare-lip. As the respiratory orifice was so disproportionately large, and the mucous membrane of the mouth so extensively exposed to the atmosphere, I considered the most urgent indication to be the limitation of the respiratory orifice, and the protection of the oral mucous membrane. The inflammation and ulceration of the latter, however, induced me to postpone the operation, as I could not assume that during its continuance the operation would be successful. I ordered the internal administration of chlorate of potassa, the mouth to be brushed carefully with an infusion of sage and mel rosæ, with warm baths of chamomile, and broth.

The following is the plan I proposed to follow in operating:—I intended forming the cutaneous septum narium with the flap of skin dependent from the tip of the nose. After its removal from the intermaxillary bone, the latter was to be extirpated, so as to convert the double, into a single, fissure. I considered the removal of the bone indicated because it was distorted and atrophic; otherwise I prefer retaining it, on account of its bearing incisor teeth. The two lateral parts of the upper lip, with the adjoining portions of the cheeks, were to be so far detached from their base, as to permit the edges of the former to be conveniently approximated to one another. In order to cause the cheeks to follow the more readily, and bring the labial halves together without unnecessary stretching, I proposed separating the cheeks and lips from the nose by an incision carried carefully round the lower boundary of the nose, through the soft parts. If after this separation there had still been much tension on attempting to bring the lids together, I should, as I have often done before, have made a vertical incision in the cheek, of sufficient length and depth, at some distance from the angle of the mouth, in order to secure greater gaping of the wound and yielding of the lip.

The red margin of the fissure was to be detached as far as the angle of the mouth, and to be turned down; the remaining part of the margin I intended simply to remove. Otherwise, the operation was to be completed as in the first case.

The operation was not, however, performed. The boy appeared much debilitated on the following day, breathed feebly, and expired in the course of the forenoon, without evincing any pain or suffering.

The following are the results of the post-mortem, which was performed eighteen hours after death:

The corpse weighed five and a half pounds, and was nineteen inches in length. There was no rigor mortis. After the scalp was removed, the coronal, sagittal, and lambdoidal sutures, as well as the parietal and occipital bones, were found to be normal. The pointed crest on the

frontal bone was caused by a complete fusion of the two halves of this bone, so as to form a projecting ridge. At this point, the frontal bone was found, on being sawn through, to be one and a half line thick. The falx was remarkably narrow, especially in front, near the crista galli; still, it penetrated to a considerable depth, so as to secure a division of the cerebrum into two lobes. The entire brain weighed eleven ounces. At the base of the brain, the course of the arteries was so far abnormal that the basilar artery gave off a communicating branch to the carotid on the right and not on the left side, and that therefore the circle of Willis was imperfect. Anteriorly, the anterior branches of the carotid anastomosed; but from the union, only one artery was given off on the left side, which did duty as the arteria corporis callosi. The posterior portions of the brain, and the origins of the nerves, were normal. The fovea Tarini was very deep, and was bounded anteriorly by two oval, shallow, small, and ill-defined mammillary bodies. Anteriorly to these lay the small shallow tuber cinereum, with a short infundibulum, and normal pituitary body.

The origins of the optic nerves, in their passage from the hemispheres over the crura cerebri, were very slightly attached. They met before the tuber cinereum in a semicircular arch, and not, as in the normal state, in an acute angle. The chiasma did not project from the cerebral tissue, but was attached to it, and did not present the crucial form. The nerves themselves, which passed off at right angles from the optic arch, were detached. They were very small, especially the one on the left. Before the chiasma, the anterior wall of the third ventricle was not formed, as usually, by the thin lamina of the corpus callosum, which passes up from the chiasma, and which above merges in the anterior portion of the corpus callosum. Instead of the normal formation, there was a commissure of grey and white cerebral substance, of one and a half line in thickness, which passed from one hemisphere to the other. Anterior to these, to the extent of about half an inch, the gyri of the anterior hemisphere, especially those corresponding to the origin of the olfactory nerves, entirely coalesced, and the olfactory nerves were deficient. Although further on the cerebrum presented a fissure of at least an inch in depth, this coalescence of gyri extended upwards, so that the anterior portion of the corpus callosum was replaced by genuine cerebral gyri, like the vermiform process of the cerebellum. Finally, these gyri posteriorly passed into a genuine corpus callosum, of one and a half inch in length, and of proper width. The posterior end was remarkable from projecting two lines above, and covering the pineal gland. The cerebellum was well developed. No abnormity was discovered at the base of the cranium, except that the alæ minores of the sphenoid bone were too short and obtuse. The orbital portions of the frontal bones were so closely pushed together, that a space of not more than two lines remained for the ethmoid bone, which was chiefly filled up with fibroid tissue. The osseous basis for the above-mentioned cutaneous flap attached to the tip of the nose, which represented the membranous septum of the nose and mesial portion of the upper lip, consisted of the vomer, with an inter-maxillary bone. The latter was only four lines thick, and therefore entirely arrested in its development. In addition to the atrophied external incisor, which had already made its appearance, the

inter-maxillary bone contained two middle incisors, of almost normal size, concealed in their alveoli; there was no trace of the right external incisor. After detaching the integuments of the face, the middle suture of the ossa frontis was found firmly united; the nasal bones, the upper maxilla, and the nasal cartilages, were well formed.

The orbits were remarkably small, especially the left one. The right eyeball was normal externally, but only five lines thick; the left only three lines in diameter, and containing a cataract. Both eyes, when examined internally, presented a peculiar malformation connected with the original development. From the internal inferior margin of the iris (the ordinary seat of coloboma iridis), some dense vascular cellular tissue, which was firmly attached to the sclerotic, passed behind the iris to the middle of the bulb, where it formed a sacculated dilatation, which enclosed the lens as a capsulo-pupillary membrane. The right lens was normal; the left one, very small, and cretified. The inner surface of the sclerotic exhibited, from the insertions of the lenticular pedicle to the foramen opticum, a thickened ridge.

No marked alterations were perceived in the remainder of the body; the trachea was well formed, the rings open, the heart sound, the thymus large; the cæcum had not descended sufficiently low, the prolongation of the peritoneum for the right testis was still open, the entire spinal cord down to the cauda equina was normal.

A review of the various anomalies of the formation of the head in the above case, shows the essential condition of the anomalies to have resided in the faulty condition of the cerebrum. This was characterized by the incomplete fusion of the anterior lobes. The imperfect separation of the cerebrum into two hemispheres, may be fairly attributed to imperfect development of the anterior termination of the central nervous system, or of the anterior cerebral lobes. The arrest of development of this lobe, from which, according to Bischoff and Von Baer, the eyes are separately produced, was also the cause of the little eyes being so closely pushed together. In the same way, the arrest of the development of the anterior cerebral lobe would entail a faulty development of the anterior portion of the chorda dorsalis. It is from this division that those portions of the nose and face are developed which, according to Huschke, advance to separate the eyes, which originally are formed from a single rudiment. We may therefore assume that the imperfect formation of the mesial portion of the face, and especially of the inter-maxillary bone, depended upon the imperfect disposition and development of the anterior end of the vertebral canal. Owing to the low degree of the arrest in the anterior cerebral lobe, the two primitive olfactory lobes and the optic lobes might in the first instance be separately developed, but would afterwards be retarded, and appear small and atrophic.

In the above case the immediate cause of the labio-palatal hare-lip may be sought in the defective development of the inter-maxillary bone. As this bone was not developed *pari passu* with the two upper jaws, its junction with them, and the union of the three portions of the upper lip, was not effected. The consequence of this was necessarily a double labio-palatal fissure.

As regards the malformation of the eyes, a peculiarity was observed which appears to offer the very opposite to what is seen in coloboma. The portion of the cutis which normally serves to form the lenticular capsule and the pupillary membrane, is folded into the orbit at its inferior and inner side, where it is surrounded by the cup-shaped cerebral portion of the bulb, so as to complete the globular form. At the point where this junction is effected we see in coloboma a thinning, whilst in the above case this point exhibited a firm vascular cellular ridge. This ridge manifestly originated in the fold of the cutis just spoken of, and, owing to morbid conditions, had persisted as a cordlike union between the lenticular capsule and its original point of entrance from the cutis.

The treatment of the red labial margin in the operation for hare-lip should seek to prevent the indentation which so frequently follows this operation, and proves a disfigurement as well as an impediment to articulation. The usual method of simply scarifying the edges of a hare-lip before uniting them, is likely to favour the formation of the indentation. Dieffenbach, who followed this method in above a thousand cases, met with the indentation at the point of union of the edges so constantly, that he remarks: "The lips are generally drawn up at this point, so that it becomes necessary to repeat the operation after some years."*

I do not venture to determine whether the indentation of the lip is due to the contraction of the cicatricial tissue, or to the fact that the latter does not yield sufficiently so as to follow the growth of the lip; both circumstances may possibly exert an influence in the matter. This secondary deformity of the upper lip has induced several surgeons to depart from the usual procedure, and make an angular cicatrix (Mirault), so as to form a projection at the point of union of the red margin. This may be done by two proceedings; either we follow Pétrequin's method, and excise the edges of the fissure by an elliptical cut—in which case, when brought together, they become elongated, and project below—or else we adopt Malgaigne's plan, who detaches the red margin of the fissure, and after bringing their two cut surfaces together, unites them by a suture. In this case, a red button is formed at the point of union.

I have followed Pétrequin's method in only two cases, but in both an indentation resulted at the point of junction of the labial border, which peculiarly disfigured the projection. I have repeatedly adopted Malgaigne's proceeding, and have still more frequently seen it employed by Professor Langenbeck, when I was senior assistant-surgeon in the surgical clinique of the university. In most cases, the cicatrix which corresponded to the union of the two halves of the lip exercised some traction upon the two sides of the button-like projection, and caused a double retraction of the red margin; in a few cases, an indentation formed in the middle of the button.

E. Mirault, of Angers, sought to prevent the indentation of the upper lip by forming a cuneiform flap at the lower edge of one portion of the lip, which was accurately fitted to the bevelled-off edge of the lower margin of the other portion of the lip. As the edges of the incision have an angular disposition, and the cicatrix, which corresponds to the point of union of the lateral halves of the lip, runs across the base of the wedge,

* Operative Chirurgie, Band i. p. 400.

the indentation cannot prove so considerable as when the edges are simply scarified before being placed in apposition. Mirault's procedure may, therefore, in many cases, secure a satisfactory result, and prevent the indentation of the lips.

It has been asserted, that when an angle is formed by the union of the edges of the wound, an indentation only ensues if union does not take place by first intention, and the cuneiform flap is retracted before union is effected. My own experience leads me to oppose this assertion. Even if the wedge is fixed by union by first intention, not only may an indentation form on either side, but the flap itself may be drawn up in the middle. I have observed this result in several cases of hare-lip operated upon either by Professor Langenbeck or myself. In my 'Chirurgische Klinik,' Band i. p. 201, I have given the case of a child (a drawing of which was taken by M. Andorff), in which Professor Langenbeck performed the operation for simple hare-lip, with an angular disposition of the cut margin, and where, notwithstanding, two indentations remained in the upper lip. They carried the well-preserved edge with them, and terminated in the direction of the nostril in a band of cicatricial tissue. In this case, however, the wound had healed by first intention.

The method which I have adopted in Case I., and which consists in making a border to the lower margin of the united lip, and forming a broad arched projection of the red margin at the point of union of the two halves, affords a better configuration of the lip than is obtained by Malgaigne's proceeding; and it causes, at the inferior margin of the lip, two horizontal cicatrices, which serve to counterbalance the contraction resulting from the vertical cicatrix. The best issue is obtained by this proceeding, when the edges of the fissure can be brought together below, in the mesial line. The projection then occupies the middle of the lip, which normally forms an arch at this point. If the union is not in the mesial line, the arched projection has to be formed lower down; as the movement of the lips is restored, and they become developed, the button is removed, and the edge of the lip becomes perfectly straight. I have in three other cases obtained as perfectly satisfactory results as in the case of Heinrich W.

In another instance it became necessary to adopt a modification of this proceeding. The patient was a girl, with double labio-palatal fissure, in whom the red margin mounted at the edge of the left half of the lip towards the floor of the left nostril, while on the right side it was only about three lines in length. The child was put under the influence of chloroform ten hours after its birth, which had occurred at the full period. After uniting the three portions of lip in the form of a Y, I bound the lip with the red margin derived from the left half of the lip alone. I turned down this red margin, and folded it under the point of union of the lateral portions of the lips, so as to form a convexity downwards. I united these two halves of the fold, the cut surfaces of which covered each other, by fine stitches. I then fastened the fold, drawing up the red margin, to the point of union of the lateral portion of the lip. That portion of the red margin of the left side which now remained and projected to the right side, was carried under the right half of the lip; its free edges were bevelled off,

and laid under, and attached to, the free bevelled end of the red margin of the right side. The result of this operation was complete. I have already had occasion to advert to the spontaneous closure of the palatal fissure which took place in this case.

Accordingly, we may detach the red margin of the lip even beyond the angle of the mouth, and make use of it to bind any part in which it may be deficient. After it is detached, it is simply drawn out and transferred to the part to which it has to be applied; it is then attached in its entire course by small stitches, both anteriorly and on the side directed towards the alveolar process. This method of transplantation I have carried out with complete success in the following case, which offers other points of interest.

I operated upon a female infant under the influence of chloroform, three hours after birth, which was perfectly normal. The labial fissure extended to the left nostril, its margins presented a very short red border. The septum narium exhibited below an arched projection, over which the tip of the nose was a little pushed up to the right. The left nostril also made an arched projection with the middle portion of its free edge, was broader, and descended lower than the right one. The child was otherwise well formed and healthy. On account of the shortness of the red border, I adopted Mirault's method. As the upper portion of the left side of the lip deviated to the left, with the ala nasi, I introduced at the point of insertion of the latter a lance-pointed straight needle, through the lower part of the nose, and surrounded it by a cotton thread, in the figure of 8 form, by which I might the more readily draw forward the left ala nasi and the adjoining half of the lip, when detached. The child breathed well after the operation, but became very restless during the following night. On the following morning the right eyelids were slightly tumefied, the conjunctiva was spotted red, and rather dry. As several cases of diphtheritic ophthalmia occurred about that time, I was afraid that we might have to deal with such an attack in this case; I therefore applied nitrate of silver, fused with two parts of nitrate of potash, neutralized with a solution of chloride of sodium and having passed over the parts a camel's-hair brush, dipped in sweet oil, I dropped in one drop of a solution of atropine (one grain to a drachm of water); compresses cooled upon ice were then applied every five minutes. In spite of the treatment, blennorrhœa, with purulent infiltration of the anterior fibrous layers of the cornea, ensued; fortunately, we succeeded in arresting the process, and to protect the left eye by closing it with adhesive plaster. The result of the operation for hare-lip was entirely frustrated by the diphtheritic process, which, as usual, affected the whole system. On the second day after the operation, the punctures and a few points of the cut edges assumed a dirty red colour and pulpy appearance, and there was no trace of adhesion. I at once removed the needle and one suture, and on the following morning was obliged to remove all the sutures, for the edges of the wound exhibited a diphtheritic condition. By energetic application of the diluted caustic, cold applications, and careful ablutions, and the internal administration of calomel, I succeeded in arresting the destruction of the edges of the fissure, but not until a portion of the red border, which had not been involved in the operation, had been attacked. Six

weeks later the upper lip was completely cicatrized, and on each lateral portion of the lip a red border of only two to three lines in length was seen. The child, under suitable treatment, recovered so perfectly, that in three months it was subjected to a second operation.

After sufficiently detaching the portions of lip from the subjacent structures, and removing the cicatricial border from the edges, I detached the red border on the left side, but extended the detachment beyond the angle of the mouth to the left half of the lower lip, so that the border sufficed to cover the lower margin of the upper lip when united. The border was distributed here as in the last case detailed of double labio-palatal fissure. As soon as the border of the upper lip was completed, I fixed the red margin in the left angle of the mouth, and then fastened it by a few stitches to the free edge of the left half of the lower lip. Throughout, healing by first intention ensued, except at the uppermost portion under the left nostril, where granulation took place. Four weeks later I excised a wedge-shaped piece, the base of which was turned forwards from the arched projection of the nasal septum, and united the edges of the wound. This little operation secured a marked improvement in the nose.

Omitting other details, the consideration of which would carry us too far, I conclude, that in operating for hare-lip the choice of the procedure to be adopted for the formation of the free margin of the lip must depend upon the relation of the red border of the fissure, if we wish to avoid indentation. The following propositions may accordingly be maintained:

1. The usual procedure of simply uniting the scarified edges of the fissure ought not to be employed at all, as the method pursued by Pétrequin certainly yields more satisfactory results.

2. If the red border of the edges of the fissure is too short to form a sufficient button under the point of junction of the lateral parts of the lip, I consider the transplantation of the red labial border, as suggested by me, to be indicated.

3. Mirault and Malgaigne's methods are inferior to the process of transplantation.

4. If the red border of the edges of the fissure suffices for the formation of a broad, arched button, beneath the point of union of the lateral portions of the lip, the method which I have proposed, of making the border to the lip with the formation of a button-like projection, promises the most satisfactory results.

PART FOURTH.

Chronicle of Medical Science.

ANNALS OF PHYSIOLOGY.

By Hermann Weber, M.D.,
Licentiate of the Royal College of Physicians, Physician to the German Hospital.

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1. Blood; Circulation; Respiration; Animal Heat.

1. Hirt: *On the Numeric Proportion of the Red to the White Blood-Cells.* (Müll. Arch., pp. 174 ss. 1856.)
2. Brunner: *On the Average Tension in the Vascular System.* (Zürich, 1854.)
3. Brunner: *On the Tension of the Blood in the State of Rest in the Living Animal.* (Henle und Pfeufer's Zeits., fur rat. Med. 1855, and Schmidt's Jahrb., vol. lxxxvii. p. 287.)
4. Becker: *On the Tension of the Carbonic Acid in the Blood, as a Measure for the Metamorphosis of the Carbonaceous Constituents of the Body and Food.* (Henle und Pfeufer's Zeits. fur rat. Med., vi. 3, 1855.)
5. Seux and Roger. *On the Pulse in New-born Children.* (L'Union Médicale, vol. ix. No. 130, 1855.)
6. Marié: *On the Relation between the Frequency of the Respiratory Movements and the Contractions of the Heart.* (Arch. Génér., Juill. 1855; and Schmidt's Jahrb., vol. lxxxviii p. 166, 1855.)
7. Vierordt and G. Ludwig: *Contribution to the Knowledge of the Respiratory Movements.* (Vierordt's Arch. fur Phys. Heilk., xiv. 2, 1855.)
8. Moleschott and Schelske: *Comparative Researches on the Quantity of Carbonic Acid excreted, and the Size of the Liver in nearly-allied Animals.* (Moleschott's Untersuch. z. Naturlehre d. Menschen u. d. Thierre, pp. 1 ss. 1856.)
9. Moleschott: *On the Influence of Light on the Excretion of Carbonic Acid by Animals.* (Wien. Med. Wochensch., No. 43, 1855.)
10. Valentin: *On the Interchange between Muscles and surrounding Atmosphere.* (Vierordt's Arch. fur Phys. Heilk., xiv. 4, pp. 431 ss. 1855.)
11. Van der Becke Callenfels: *On the Influence of the Vaso-motor Nerves on the Circulation and Temperature.* (Henle und Pfeufer's Zeits. fur Rat. Med., vii. pp. 157 ss. 1856.)
12. Kussmaul and Tenner: *On the Influence of the Circulation in the Large Vessels of the Neck on the Temperature of the Ear, and its Relation to the Alteration of Temperature by Paralysis and Irritation of the Sympathetic Nerve.* (Moleschott's Untersuch. z. Naturlehre d. Menschen u. d. Thiere, i. pp. 90 ss. 1856.)

Hirt, who made his observations principally on his own blood, found the number of the white globules always considerably increased from half an hour to one hour after each meal; the influence of the latter in general disappeared two hours later (i.e., two and a half to three hours after the end of the meal). Thus, at 8 A.M., when breakfast was taken, the proportion of the white to the red globules was = 1 : 1760; from half-past 8 to 9, = 1 : 700; between 9 and half-

past 10, it sank to 1 : 1510; at 1 P.M. (dinner) = 1 : 1510; from half-past 1 to 2 P.M. = 1 : 420; at half-past 3 P.M. = 1 : 1480; at 8 P.M. (supper) = 1 : 1480; at half-past 8 P.M. = 1 : 550; between half-past 10 and half-past 11 = 1 : 1230; from then to 6 A.M. it sank gradually to 1 : 1760. Hirt agrees with Funke and Vierordt, that the blood of the vena lienalis is much richer in white cells than that of the artery; but according to him the proportion is only = 1 : 60, while Funke asserts that the white cells sometimes form the fourth part of the whole number; and Vierordt described it in the vein of a decapitated criminal (one hour and a half after death) as large as 1 : 4,9. The author corroborates Lehmann's statement, that the blood of the hepatic vein contains a larger amount of colourless corpuscles (1 : 136) than that of the vena portæ (1 : 514). Some experiments with tonic remedies lead him to the inference that these possess the power of increasing the number of white globules in a very remarkable manner; the tincture of myrrh being in this respect superior to that of bark, and still more to that of the malate of iron.

Brunner measured the tension of the blood in the vessels by means of C. Ludwig's "Kymographion." He endeavoured to arrest the motion of the thorax, the limbs, and the heart, by placing the animals under the influence of opium or chloroform, and of electrifying at the same time the peripheric ends of the dissected vagi. We must refer for the details to the original communications; stating here only, that from the author's experiments it becomes evident, that the blood is under a considerable degree of pressure, also when in a state of rest. The cause of this is attributed to the circumstance that the capacity of the blood-vessels, when their walls are not stretched, is smaller than the bulk of the blood contained in them; that the vessels must therefore be distended in order that the blood may find room within them. The degree of tension varies much in the same animal: (*a*) it becomes increased by the quantity of the blood being increased, (*b*) diminished in the opposite case; thus, the tension in a small dog was found to be at first equal to 10·4 millimètres of mercury, it rose to 19·0 mm. after the injection of 280 grammes of blood, and fell later to 8·5 mm. when 356 grammes of blood had been detracted. As the quantity of the blood is frequently changing, according to the state of health, the meals, and other influences, it is without doubt that the degree of tension is not always the same. The quantity of blood remaining the same, its tension varies with the expansibility of the vessels, the amount of contraction or relaxation of the surrounding muscles, the position of the limbs, &c. In Brunner's experiments the tension varied during the continuance of the irritation of the vagus between 4 mm. and 29 mm.; but, however long he applied the irritation, he never succeeded in reducing the state of tension to so low a grade as it assumed at the moment of death. The power of resistance in the walls of the vessels must therefore undergo a change while life ceases. The influence of the irritation of the vagus shows itself almost immediately in the arteries, but only after the lapse of some seconds in the veins.

Becker's method appears to be more simple than any one of those hitherto employed. The air is expired into a glass bell placed over mercury; the carbonic acid is determined by Bunsen's method (eudiometer and balls of potassium). The inspirations and expirations were of course made, in Becker's observations, according to a certain rule, sixty seconds being the time during which the breath was retained after a profound inspiration, whenever no intentional deviation is mentioned. The principal results are:—1. That the tension of the carbonic acid in the blood varies; that it increases and decreases with the quantity of carbonic acid contained in the blood, which is proportionate to the quantity of carbonic acid expired in a given space of time. 2. That the carbonic acid is expired in varying quantity, according to the length of time during which the breath is retained. We will quote the results of Vierordt's, as well as Becker's, experiments on this subject:—

		Becker.	Vierordt.
Time of retention	0 seconds	... 3·636 per cent. CO_2	... —— per cent. CO_2.
" "	20 "	... 5·552 "	... 4·80 "
" "	40 "	... 6·265 "	... 5·21 "
" "	60 "	... 7·176 "	... 6·06 "
" "	80 "	... 7·282 "	... 6·44 "
" "	90 "	... —— "	... 6·50 "
" "	100 "	... 7·497 "	... 8·06 "

3. While the *temperature*, and the *frequency of pulse* and *respiration*, reach their maximum immediately, or at least within the first hour, after the principal meal, the tension of the *carbonic acid* becomes greatest after two or three hours, and *urea* is excreted in the largest proportion from the second to the fifth hour following the meal. 4. Concerning the *period of the day*, we find the tension considerable (6·904 p. c.) at 6 A.M.—i.e., immediately after waking; then, no food being taken, it decreases until 10 A.M. (6·287 p. c.); after this it rises, to reach its maximum at about 2 P.M. If a meal is taken at noon, the tension begins immediately afterwards to rise more considerably, reaching its highest degree within two hours and a half (7·593 p. c.) When no food was taken, Becker found this increase less marked (only to 6·89 p. c.); but, in opposition to Vierordt, he asserts that it is never altogether absent, and that it coincides with the increase of temperature and frequency of pulse, which are likewise observed at that time, independently of the ingestion of food. After about 3 P.M. the tension begins again to diminish. 5. Increased consumption of water has scarcely any influence on the tension of the carbonic acid, while the excretion of urea becomes remarkably augmented. Thus Becker shows that "as the elimination of carbonic acid is considerably influenced by the quantity of air inspired, that of urea is to some degree dependent on the ingestion of water."

Seux gives the results of his own and Dr. Magail's observations on the pulse of infants, from the period commencing directly after birth, and ending at the age of two months. These observations, which are made at the Charité Hospital at Marseilles, and for the greatest part corroborated by Dr. H. Roger, at the Hospital for Children in Paris, lead to the following inferences:—1. The pulse of infants, when in the state of health and quietude, may vary from 80 to 164. 2. In the greater number, it ranges between 120 and 140; then follow the cases between 140 and 160; afterwards, those from 100 to 120; later, those above 160; lastly, those below 100. 3. It is in general regular; sometimes, however, several pulsations follow each other more quickly, and are succeeded by others which are separated by a longer interval. These irregularities were found in cases that were below the average frequency. 4. The sex, constitution, salubrity of residence, or time of the year, appear to exercise no influence. 5. The pulse is more frequent during the first few hours after birth, but from the end of the first day to that of the second month, no difference is to be attributed to the age. 6. The periods of the day are without influence. 7. The act of sucking in general quickens the pulse, which influence remains perceptible during about half-an-hour. 8. Sleep and waking, quietude and agitation, have a marked influence. During sleep, the frequency is diminished, it rises when the child awakes but remains calm, and still more when it becomes agitated. Thus it rose in one instance at first from 104 to 120, and afterwards, when the child began to cry, to 134. A sudden effort or emotion may cause an increase of 20 or 25 beats in a minute.

It will be seen that these observations confirm those of Knox, Guy, Valleix, and others, in most points, but that they differ in their statement on the influence of the period of the day and of the sex.

Marié gives 20 as the average number of the *respiratory movements*, 71 as that for the *pulse;* the proportion of the former to the latter, 1 to 3·51. For women, he found the figures 77 and 23; for men, 69 and 19. The proportion between the two increases and decreases with the frequency of the pulse, in support of which

inference, the following table is compiled from the examination of patients of different nature:—

Average number of respirations.		Average frequency of pulse.	Proportion.		Number of cases.
16	...	43	1 : 2·69	...	12
19·73	...	53·6	1 : 2·71	...	15
24·7	...	70 E	1 : 2·83	...	93
24·7	...	82	1 : 3·32	...	39
35·	...	104	1 : 2·97	...	54
42·43	...	142·28	1 : 3·35	...	21
50	...	172	1 : 3·4	...	4

Hourmann and Déchambre arrived at very similar results in their observations on aged persons. Exceptions to this rule are in general found after strong mental emotion, when the respiration mostly remains calm, while the frequency of the pulse becomes considerably increased; further, after meals, which cause about eight pulsations more in the minute, although the number of respirations is rarely influenced. Several pathological conditions, and the effect of digitalis, may likewise be named among the exceptions.

Vierordt and G. Ludwig performed their experiments on five male subjects, aged 36, 20, 51, 34, and 7 years. Concerning their method, we must refer to the original essay; we only mention here that the respiratory movements were measured on a point of the linea alba, a little below the umbilicus, and represented by means of a lever-like instrument on the Kymographion drum.

The principal inferences are—1. The duration of the *single* respirations in the same experiment (three to six minutes) varies considerably; the average duration drawn from the lowest figures in the five individuals to that from the highest, bears the proportion of 100 to 209, although the experiments are made in the state of quietude. 2. Each respiratory movement is divided into four periods,—*a*, inspiratory period; *b*, inspiratory pause; *c*, expiratory period; *d*, expiratory pause. The duration of the inspiratory period varies in the average from 100 to 232, that of the expiratory period from 100 to 226. For the relation of these four periods among themselves, the authors use the term *celerity* of respiration; *quick* they call a respiration with a short inspiratory period; *slow*, the opposite. The inspiratory period signified by 10; the expiratory period occupied in the quickest respiration, 38; in the slowest, 12. The respiration is very quick during reading aloud. The inspiratory pause is very short—frequently altogether wanting; the expiratory pause bears on the average to the whole respiratory movement the proportion of 10 : 44. 3. Another series of experiments exhibit the relation between the respiratory movements and the state of repletion of the lungs with air. The authors arrived at the following results:—*a.* The more calm the respiration, the less replete are the lungs with air, and *vice versâ*. 100 being the figure for the vital capacity, 19 3 corresponded in an average of nine experiments to the lowest point at the commencement of the inspiration, while during excited respiration, 56 9 was the lowest reading. *b.* The repletion of the lungs during the state of calmness is such, that the quantity of air changed by each respiratory act is about one-third greater than that which remains in the lungs, and might be expelled by forced expiration. *c.* The average repletion of the lungs was 33·5 per cent. of the vital capacity. Experiments with the spirometer led to the same result. *d.* The repletion of the lungs at the commencement of each inspiration varies much in the same experiment. *e.* When the state of repletion is lower, the inspirations are more profound than when it is higher.

Moleschott and Schelske employed in their experiments the Rana esculenta, and several nearly allied genera and species of animals. The following inferences are of physiological interest:—1. The batrachians yield, in proportion to their weight, less carbonic acid than man, but when they breathe in air charged with moisture, the difference is not so great as is generally assumed. If we adopt, with

Donders, 1593 milligrammes of carbonic acid within 24 hours, for 100 grammes of man, as the unity, Rana esculenta yields 0·37; Hyla arborea, 0·39; Triton cristatus, 0·63; Rana temporaria, 0·69; Bufo cinereus, 0·25; Bufo calamita, 0·37. 2. Several species of the same genus exhibit a wide difference in the proportionate quantity of carbonic acid excreted and in the size of their livers (weight of liver for 100 grammes weight of body in Rana esculenta, 6·0 grammes; in Rana temporaria, 3·37 grammes). It is therefore unsafe to transfer the laws of metamorphosis of matter found for one species to another one, although nearly allied. 3. Among the nearly allied animals, the most inert (Bufo cinereus, Salamandra maculata) yield the smallest, the most active (Rana, Bufo Calamita, Hyla, Triton) the largest amount of carbonic acid. 4. The comparison between Hyla arborea and Hyla esculenta shows that between nearly-allied animals, living more in the air and less in the water, yields the larger proportion of carbonic acid. 5. The experiments made on the two sexes separately corroborate (with the exception of Triton) the law found already by Andral and Gavarret, that the male sex produces, for the same weight, more carbonic acid than the female; the proportion in Bufo cinereus $=1·43:1$, in Rana esculenta and temporaria$=1·28:1·6$. 6. The liver can in no way be considered as a measure for the excretion of carbonic acid in different species of animals.

In another series of experiments on the influence of light—made likewise on frogs—Moleschott arrived at the following results:—1. Frogs excrete, the temperature being the same, or nearly the same, for an equal weight of body, considerably more (from one-twelfth to one-fourth of the whole amount) carbonic acid when under the influence of light, than when kept in the dark. 2. The more intense the light, the greater the amount of carbonic acid excreted. 3. The light appears to exercise its influence partly through the eyes, partly through the skin.

Valentin's researches, made on the muscles and other parts of the Rana esculenta, manifest that there is a constant interchange between the muscles and surrounding atmosphere, and that the muscles endowed with irritability and those deprived of it, exhibit a great difference in this respect. The difference becomes evident as soon as the muscle is dead, whether this death is caused suddenly, as by cold, heat, mechanical injury, &c., or gradually, as in the amputated limb. Again, a difference is perceptible in the action of the dead muscle on the surrounding atmosphere, according to the manner in which the death has been effected (cold, heat, mechanical power, &c.). The difference between the action of the living and that of the dead muscle increases with the advancing decomposition of the latter. The living muscle produces a considerable diminution of the volume of the surrounding atmosphere; this diminution becomes less evident as soon as the irritability ceases; by degrees it becomes equal to zero, and during putrefaction an increase of volume is observed. These changes are caused principally by the absorption of oxygen, the development of carbonic acid and nitrogen. Concerning the *oxygen*, Valentin remarks, that as well the living as the dead muscle absorb more than is proportionate to the carbonic acid given off by them. Not only the muscles, but also the other tissues, exercise an influence on the surrounding atmosphere, by absorbing oxygen and yielding carbonic acid. The quantity of the *nitrogen* undergoes only slight alteration, or none at all, while the irritability of the muscle persists, but development of nitrogen takes place as soon as the decomposition commences.

Callenfels considers—*a*. The nature of the periodic contractions and dilatations of the arteries of the ear of rabbits, lately described by Schiff. He admits the existence of this phenomenon, but could not observe the regularity and frequency, as represented by Schiff. While the latter had seen from two to six periodic changes in a minute, Callenfels found each change occupy a whole minute or more. During cold weather, the state of contraction continued sometimes even for hours; while in warm weather, the dilatation predominated. A close relation existed always between the lumen of the vessels and the temperature of the ears, the latter

being low during the contraction, high during the dilatation, of the vessels. He is inclined to ascribe, from his observations, to the ears of the rabbit the function of husbanding the temperature of the animal, by giving off much warmth when the vessels are dilated, and little when they are contracted.

b. The experiments on the *sympathetic nerve* on the neck lead Callenfels, on the whole, to similar results as those obtained by Bernard and others;* he found, however, in opposition to Bernard, that mere section of the nerve exercises a greater influence on the temperature than extirpation of the ganglion supremum. And again, in contradiction to the same author, he states that "the connexion between the dilatation of vessels and the temperature is so close, that we can almost ascertain the temperature of the ear by mere inspection."

c. Concerning the vessels of the pia mater, Callenfels obtained, after many negative results, the decided proof, that the arteries of the pia mater are under the influence of the sympathetic nerve on the neck. Irritation of the nerve produced distinct contraction of the small arteries on the same side; discontinuance of the irritation was followed immediately by dilatation of the same vessels.

Kussmaul and Tenner draw, from their ingenious experiments on rabbits, the inference, that the *sympathetic* nerve has no direct influence on the formation of animal heat, as Bernard had concluded; but that it acts merely through the coats of the bloodvessels, by allowing a larger or smaller quantity of blood to enter into them. Our authors adopt, therefore, the mechanical view propounded by Donders, Schiff, Callenfels, and others. They do so, on the following results of their experiments:—1. By direct increase of the supply of blood, the same increase of temperature of the ears may be effected, and even a greater one, than by paralysis of the sympathetic nerve. 2. By direct arrest of the supply of blood, the same degree of lowering of temperature may be obtained as by irritation of the sympathetic nerve. 3. The differences in the temperature of both ears, effected through the arrest of the supply of blood on one side, are as great as those produced by paralysis or irritation of the sympathetic nerve on one side. 4. The arrest of the supply of blood on *one* side causes increased redness and warmth in the ear of the opposite side, just as irritation of the sympathetic on *one* side. 5. After section of the sympathetic nerve, the temperature of the ear can be still increased by increasing the lateral pressure in the vessels. 6. In the ear deprived of blood, the decrease of temperature continues in spite of section of the sympathetic nerve. 7. The temperature in the ear sinks more rapidly in consequence of arrest of the supply of blood, if the sympathetic nerve has been previously cut.

II. Lymphatic System and Ductless Glands.

1. Krause: *Contribution to the Physiology of the Lymph.* (Henle and Pfeufer's Zeitsch f. rat. Med., vii. pp. 148 ss.)
2. Fuhrer and H. Ludwig: *On the Physiological Compensation of the Spleen, and on the Sources of Urea.* (Vierordt's Arch., xiv. pp. 307 ss. 1855.)

Krause collected the lymph from the truncus lymphaticus cervicalis of dogs, previously narcotized by opium. His experiments show—1. That the quantity of lymph yielded by one kilogramme of the dog, in the state of fasting, fluctuated between 246 and 638 grammes, the average being 435 grammes; 2. That it is not materially reduced by diminished tension in the arterial system (tying of the carotids); 3. That it is increased by irritation of the sensitive nerves; 4. That the composition of the lymph varies considerably; 5. The reaction was in all cases slightly alkaline; 6. The existence of *leucin* (Staedeler and Frerichs) could not be distinctly proved; 7. The presence of sugar (of milk or grapes) became, by the use of Trommer's test, very probable.

* British and Foreign Medico-Chirurgical Review, No. 33, p. 231. 1856.

Führer and H. Ludwig corroborate the result, already obtained by other experimenters, that the lymphatic glands become considerably enlarged after the extirpation of the spleen. The glands of the abdomen, chest, and neck exhibited the greatest increase; while those of the legs and inguinal regions were little altered. The convexity of the upper and the concavity of the lower surface of the hypertrophied glands were very marked, the veins issuing from the hilus much increased in diameter, the lymphatic vessels exhibiting only the usual size. The authors attribute the enlargement principally to the exaggerated development of the glandular bodies (Brucke, Donders, Kölliker), which appear to be surrounded by a close network of newly-formed capillaries, analogous to those described in the spleen.* Fuhrer and Ludwig are of opinion that the lymphatic glands, thus developed, exercise, instead of the spleen, the function of forming blood-corpuscles.

Concerning the final destination of the blood-globules, the authors entertain the view, that they form the principal source of urea. They thus object as well to the theory of Liebig and Bischoff, that the urea is the production of the "metamorphosis of matter of the solid tissues" (this being, according to Führer and Ludwig's view, far too steady and too slow to account for the frequent changes in the quantity of urea, and its enormous and rapid increase, in consequence of increased ingestion of azotized food); as also to that of Frerichs, Bidder and Schmidt, who maintained that a part of the urea was derived directly from the metamorphosis of the overplus-consumption—i.e., that part of food which is taken over and above what is required for the conservation of the system. Führer and Ludwig contend that, in the same way as bile, milk, and mucus are not excreted directly from the blood, but through the intermediate formation of cells, thus also urea is eliminated by means of cells—namely, the blood-globules. As favourable to this opinion may be mentioned the rapid development, short duration, and frequent change of the blood-globules, as also the absence of any other known metamorphosis of these bodies.

III. Secretion; Metamorphosis of Matter.

1. Boedeker: *On the varying Composition of the Milk at different Times of the Day.* (Henle und Pfeufer's Zeits. f rat. Med., vi. 2, 1855.)
2. C. G Lehmann: *Communication concerning the Question of the Formation of Sugar in the Liver.* (Schmidt's Jahrb., vol. lxxxvii. pp. 282 ss 1855.)
3. Ch. Bernard: *Sur le Méchanisme de la Formation de Sucre dans le Foie.* (L'Union Méd., No. 119, vol. ix. 1855.)
4. Limpert and Falck: *Researches on the Excretion of Sugar through the Kidneys, when Injected into the Blood.* (Virchow's Archiv, ix. 2, pp. 56 ss. 1856.)
5. H. Nasse: *On the Section of the Vagi*—see under the head of *Nervous System.*

The milk examined by Boedeker was that of a cow in good state of health, fourteen days after calving. The cow was fed between 6 and 10 A.M., and between 5 and 8 P.M., with hay, oat-straw, beet-roots, oil-cakes, and ground beans; it had an additional meal of oat-straw at 10 P.M.; it was milked at 4 A.M., at noon, and at 7 P.M. Leaving the chemical details to the Report on Physiological Chemistry, we give here only the principal results of repeated examinations:—1. The percentage of *fat* was smallest in the morning milk, larger in the milk of noon, and largest in that of the evening, which bore to that of the morning milk the proportion of 2 to 1—viz., sixteen ounces of milk obtained in the evening contained six drachms of butter, of milk obtained in the morning only three drachms. 2. *Casein* was likewise increased in the evening milk, but not in so considerable a degree as fat. 3. The quantity of *albumen* was diminished in the milk of the evening in very nearly the same proportion as the casein was increased. 4. The

* British and Foreign Medico-Chirurgical Review, vol. xxvii. p. 253.

sugar of milk and the *salts* exhibited only slight variations at the three periods. These facts show again, how necessary it is, to examine the secretions and excretions of the body at different periods of the day, in order to obtain an accurate knowledge of their composition.

Lehmann communicates the results of several new experiments, performed, partly on horses, killed five hours after the last meal; partly on dogs, of which some had been killed in the fasting state, others five hours after a meal of raw meat, others after one consisting of boiled potatoes. The blood of the portal vein of the dogs, killed in the state of fasting, and of those fed with raw meat, contained no sugar; that of the horses, and of the remaining dogs—viz., those fed with potatoes, only a small quantity; while the blood of the hepatic vein exhibited in all cases a very large amount. Lehmann attributes the origin of the sugar formed in the liver, in part at least, to the fibrin and albumen, the proportion of which is diminished in the blood of the hepatic vein. The author's repeated experiments confirm again the fact pointed out by Bernard, that the arterial blood is usually free from sugar, that only when the venous blood in the right ventricle contained 3 per cent. of sugar or more (viz., in cases of diabetes mellitus), a part of the latter passes into the arterial blood. Poggiale* has recently arrived at results very similar to those of Lehmann, concerning the proportion of sugar in the blood obtained from different vessels. Leconte,† too, found in his experiments on dogs no sugar in the blood of the portal vein after meals of meat, while the blood of the hepatic vein contained 0·1 to 0·4 per cent.

Bernard took the liver of a dog, fed exclusively on meat, immediately after death by section of the medulla oblongata; he washed it out by a continued stream of water through its vessels, so completely, that it was quite exsanguious, and that the decoction of a piece of it did not any more yield a trace of sugar. When he examined the remainder of this liver after twenty-four hours, he found it to contain a very large amount of saccharine matter. From this simple experiment, frequently repeated with the same result, Bernard concludes, that the sugar is not formed, as other physiologists have suggested, by a kind of catalytic action, exercised by the glandular tissue on a constituent of the blood, while it circulates through the liver, but by the metamorphosis of a substance contained in the tissue of the liver itself. The same experiment shows, that this substance, which is to be transformed into sugar, is insoluble in water; the author has further convinced himself, that it is also insoluble in alcohol and ether; that its transformation into sugar is, in general, terminated within twenty-four hours; that it is accelerated by the free exposure to the atmospheric air (as by cutting the liver into very small pieces); that, on the other hand, this faculty of undergoing the transmutation in question is destroyed by the process of boiling. Bernard remarks, that, in the state of health, this substance is constantly reproduced in the tissues of the liver, and as constantly afterwards transmuted into saccharine matter.

Limpert and Falck injected milk-sugar, grape-sugar, and cane-sugar into the jugular veins of dogs, in solutions containing from 5 to 13 grammes of the one or other kind of sugar. The urine was obtained by means of the catheter. The principal results of their experiments are—1. Of 5 grammes of *sugar of milk* injected in 4 cases, 2·6 grammes were excreted with the urine in two cases, 2·04 grammes in the third, 3·36 grammes in the fourth. The excretion of sugar with the urine had in all cases ceased after five hours. 2. Of 5 and 7 grammes of *grape-sugar*, only very slight traces were found in the urine, and even these traces only within the first couple of hours after the injection. 3. Of 10 and 13 grammes of *grape-sugar*, 1·45 grammes and 0·2 grammes were excreted within the first five hours—none later. 4. Of 8 grammes of *cane-sugar* injected in two cases, 5 grammes passed through the kidneys in one of them, 4·87 grammes in the other. This elimination was in both cases completed within seven hours. 5. Of each kind of

* Gazette de Paris, Nos. 17, 18. 1855.
† Annales des Sciences Naturelles, pp. 61 ss. 1855.

sugar, therefore, when injected in a considerable quantity, a part quitted the body through the kidneys, which part was largest for cane-sugar, smaller for milk-sugar, smallest for grape-sugar. It will be seen from this statement that the results obtained by Limpert and Falck are in accordance with those obtained by Bernard, Lehmann, Kersting, Uhle, and Becker. They differ from all of them, by taking into consideration the exact quantity of sugar secreted by the kidneys, and in common with only those of Becker, they give account of the time within which sugar is eliminated in this way.

IV. Nervous System.

1. Marmé and Moleschott: *On the Influence of Light on the Irritability of the Nerves.* (Moleschott's Untersuch., pp. 15 ss. 1856.)
2. Brown-Séquard: *Experimental and Clinical Researches on the Physiology and Pathology of the Spinal Cord and some other Parts of the Nervous Centres.* (Richmond, 1855.)
3. Brown-Séquard: *Experimental and Clinical Researches upon the Channels of Transmission of the Sensitive Impressions through the Spinal Cord and Medulla Oblongata.* (Med. Times and Gaz., Nos. 304-5, 1856.)
4. Pflüger: *On the Influence of the Anterior Roots of the Spinal Marrow on the Lumen of the Vessels.* (Preliminary Communication. Med. Centr. Zeit., Nos. 68 and 76. 1855. And Schmidt's Jahrb., vol lxxxix. No. 1, 1856.)
5. H. Nasse: *On the Influence of Section of the Vagi in Dogs, with special regard to the Metamorphosis of Matter.* (Arch. f. Wissensch. Heilk., vol. ii. 3, 1856.)
6. Luschka: *On the Sensitive Branches of the Nervus Hypoglossus in Man.* (Müll. Arch. i. and ii. pp. 62 ss. 1856.)
7. Th. Weber: *On the Want of the Sense of Touch in Parts Denuded of the Cutis.* (Vierordt's Arch f. Phys. Heilk., xiv pp. 311 ss. 1855.)
8. Pflüger: *A System for Arresting the Peristaltic Movements of the Intestines.* (Monatsber. d. Berlin Akad., and Schmidt Jahrb., vol. lxxxix. No. 1, 1856.)

Callenfels', and Kussmaul's, and Tenner's Essays on the Sympathetic Nerve, are reported on under the head of "Animal Heat."

Marmé's and Moleschott's experiments prove distinctly that frogs kept in the light possess a higher degree of irritability of the nerves, and greater power of the muscles, than such that have been deprived of the influence of light; the sex, the size of the body, the state of nutrition, the time of the year, and the temperature, were of course in both cases the same.

Brown-Séquard has continued his ingenious researches on the physiology of the spinal marrow. We must restrict ourselves for the present to the most important results; the more so, as the author promises a detailed account in his 'Physiology and Pathology of the Spinal Cord,' which he is about publishing in Paris. Physiological experiments, together with pathological facts, lead Brown-Séquard to the following inferences:—1. The idea that the sensitive impressions are conducted to the encephalon along the posterior columns, is entirely erroneous. 2. The grey matter of the spinal cord, although itself deprived of sensibility, is an organ of transmission of the sensitive impressions. 3. There are two kinds of sensitive fibres in the posterior columns of the spinal cord, some going up towards the encephalon (centripetal or ascending fibres), some going in the opposite direction (centrifugal or descending fibres). 4. There are also ascending and descending fibres in the posterior grey horns, and very likely in the posterior parts of the lateral columns. 5. These ascending and descending fibres in the posterior columns come mostly, if not entirely, from the posterior roots of the spinal nerves. 6. The posterior roots send also fibres to the posterior grey horns, and very likely to the posterior parts of the lateral columns. 7. All these fibres soon leave the posterior

columns, the posterior grey horns, &c., in order to go into the central grey matter. 8. All these sensitive fibres decussate very near their entrance into the spinal marrow from the posterior roots. 9. There are some transverse fibres in the spinal cord, coming from the posterior roots, which do not seem to transmit sensitive impressions. 10. The *motor* nerves remain, after their entrance into the spinal marrow, on the same side, until they reach the lower part of the medulla oblongata, where they decussate.

Pflüger made his observations on frogs. By applying at first weak, then gradually-increasing, currents on the anterior roots of the crural nerves (by means of Du-Bois-Reymond's apparatus), he constantly effected contraction of the arteries of the web-membrane. Sometimes even the largest arteries of the web became so completely contracted, that all the blood-globules disappeared from them, and this took place principally in a retrogressive direction. The greatest degree of contraction took place in general about ten or fifteen seconds after the commencement of the irritation. The same observation has been also made on the mesentery of the frog. Regarding the influence of irritation of the spinal marrow on the veins, the author promises further communications.

On the effect of *section of the vagi*, Nasse has performed a series of valuable experiments. Particular attention has been paid to the influence upon the animal temperature, the frequency of pulse, the respiration, the composition of blood and urine, the loss of weight, &c. Section of *only one* nerve causes some functional disturbance, which, in general, is only transitory—as increased frequency of respiration, diminished frequency of the contractions of the heart, increased secretion of saliva, &c. The most constant effect, however, is considerable emaciation, in spite of abundant ingestion of food. The blood shows increase of albumen and water, decrease of the number of blood-globules. The quantity of fæces increased, less digested; the urine contains a diminished amount of solids; perspiration above the average. Death was never caused by the section of one pneumogastric nerve; but when the animals were killed some days after the operation, they always exhibited hyperæmia of *both* lungs. Section of *both vagi* always proved fatal between the second and sixty-second day after the operation. The principal phenomena were:—1. Diminished number of respiratory movements: in the average of six cases, they fell from 18·1 to 12 soon after the operation, and to 5·6, in one case even to 3, within eight days. 2. Increased frequency of contractions of the heart, in the average from 113 to 165 (viz., about 38 per cent.). 3. Increased impulse of the heart. 4. As regards the pressure of the blood, Nasse's experiments gave not so constant a result as those of other observers, who contradict each other—(Bernard contends that it is diminished, Ludwig that it is increased);—his result is most in accordance with that of Lenz, who found the pressure above the standard soon after the section of the nerves, below it in a later period. 5. *Vomiting* in all cases, frequently without ingestion of food. 6. *Desire for food* at first rather increased, gradually decreasing, sometimes altogether lost. Thirst excessive. 7. *Digestion* much impaired, principally that of meat; milk, bread, and fat in small quantities are better borne. The alvine dejections are increased in quantity, and very offensive. The gastric juice not quite deprived of acid, but the latter much diminished, as also the pepsine. The absorption of poisons undisturbed. 8. The *temperature* in general below the average during the first days, rather above it after the fourth day, sinking again below it one or two days before death. 9. *Blood*-globules, albumen, and fibrin in excess; water in diminished proportion. 10. The *loss of weight* is much greater than in animals merely deprived of food; the author ascribes this in a great measure to the excess of perspiration and secretion of urine. The daily loss of weight was greatest in those animals that died soonest after the operation, but the total loss was largest in those which lasted out longest. Concerning the post-mortem phenomena we must refer to the original; we mention, however, the interesting fact, that the liver was found to contain no sugar, either immediately after death or a few days later (Bernard), with only one exception.

Although Luschka maintains the entire absence of a posterior ganglionic root in the nervus hypoglossus of man, yet he thinks that we are compelled, by the distribution of its branches, to admit its mixed nature. The anatomical examination teaches that the source of the sensitive fibres cannot exist in the origin of the nerve, it must therefore be looked for in its course from the centre to the periphery. The author proves that the sensitive elements cannot be derived from the communication with the sympathetic nerve, as only the latter receives motor fibres from the hypoglossus (iris-sympatheticus of Budge); he further shows, that the connexion with the cervical nerve is only transitory, and that with the vagus inconstant. Luschka then finds the source of the sensitive fibres in the *ramus lingualis* of the fifth pair, and in the ganglion sublinguale, from which some fibres constantly join the hypoglossus, and run along its trunk backwards to the place where they are distributed to the periphery. The sensitive elements thus obtained are distributed to the occipital bone, the vena jugularis, the sinus circularis of the foramen magnum, and to the circellus venosus hypoglossi—a circular sinus-like arrangement of veins, with very delicate membranes round the nervus hypoglossus at its entrance into the canalis nervi hypoglossi.

Th. Weber performed his experiments on a patient in whom the cutis and the sub-cutaneous cellular tissue had been recently destroyed on a great part of the right arm and fore-arm, in consequence of inflammation and gangrene of the subcutaneous cellular tissue; the muscles were denuded to a considerable extent. These experiments show that muscles deprived of skin do not possess the faculty of perceiving heat and cold, high degrees of warmth being perceived not as temperature, but only as pain; they further confirm, that the sensibility of muscles is in every respect less acute than that of the skin; the smallest distance between the points of the compass to be felt as two, amounted, in the longitudinal direction, to ten centimètres.

Pflüger communicates the very interesting discovery, that *irritation of the nervi splanchnici* arrests the motion of the small intestines. We have therefore, it appears, another instance of the phenomenon, that the increased action of a nerve stops the motion of a muscle. Pfluger draws his inference from the following experiments:—1. One of the electrodes of the apparatus was applied to the denuded muscles of the back of a rabbit, between the fifth and sixth, the other to those between the tenth and eleventh vertebræ. As soon as the apparatus was put into action, the trunk and extremities became tetanized; while at the same time the peristaltic motion of the small intestines ceased altogether; the colon and rectum continued to move. The removal of the electrodes was immediately followed by the recommencement of the peristaltic motion. 2. The peristaltic movement of the intestines does not cease, if in the preceding experiment the splanchnic nerves have been cut through before the electricity is applied. 3. Application of the electricity to either of the splanchnici alone is sufficient to effect the cessation of the peristaltic motion of the small intestines.

HALF-YEARLY REPORT ON MATERIA MEDICA & THERAPEUTICS.

By EDWARD BALLARD, M.D.,
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I. *On Cantharidin, and its relation to Spanish Flies.* By Dr. SCHROFF. (Zeitsch. der k. k. Gesellsch. du Aerzte zu Wien. July and Aug. 1855.)

COMPARATIVE experiments were made upon rabbits with the cantharidin and Spanish flies, and one comparative experiment by M. C. Heinrich, who took at one time 10 drops of a strong tincture of cantharides, prepared by himself from fresh undried flies; and at another time, 0·01 gramme of cantharidin. It is clear from these observations that the cantharidin is the irritating principle of the flies, as it not only produced gastro-enteritis, but, after absorption, also proved irritant to the urinary organs. One interesting result obtained by Heinrich is, that the cantharidin, although producing inflammation along the whole digestive tube, and in the urinary organs, failed to produce any excitement whatever of the sexual system, while the latter was a marked effect of the tincture of cantharides. The facts in our possession point to the volatile principle in the living flies, which gives them their disagreeable odour, as that which most rapidly occasions sexual excitement.

II. *On Lactate of Zinc in Epilepsy.* By M. HERPIN. (Bull. Gén. de Thérap. Nov. 1855.)

M. Herpin points out the fallacy of deductions from cases treated *en masse* by any remedy, without classifying the cases, and taking the prognosis into consideration. He divides cases of epilepsy into three groups. 1. Where the prognosis is *favourable*. This embraces cases in which there have been less than 100 attacks. 2. *Little favourable* cases, where there have been from 100 to 500 attacks. 3. *Unfavourable* cases, where there have been above 500 attacks. The duration of the affection, together with the age and sex of the subject, also influence the prognosis. All things being equal in respect to the number of fits, the most recent cases are the most favourable. Under five months' duration, the chances of recovery are twice as great as from five months to a year. After ten years, success is rare. Of all ages, old age is the most favourable; then youth and infancy; and least of all, adult age. In M. Herpin's hands there have been twice as many failures with males than with females. Adult men are most unfavourable subjects. To apply this sort of division to the cases treated by lactate of zinc:—of 41 epileptics, the treatment was only sufficiently advanced in 35 for any decision as to its effect being arrived at. Of these 35, 15 were favourable cases, 12 little favourable, and 8 unfavourable. Of the 8 unfavourables, 2 have improved to an extent which militates strongly in favour of the remedy. Of the 12 little favourable cases, in 2 children, aged respectively eight years and twenty-one months, the fits were suppressed, and a remarkable amelioration took place in one man. Of the 15 favourable cases, 4, in which various other remedies had failed, were uninfluenced by the lactate; one of these had suffered 90 attacks in fifteen years and a half; a second, aged forty-four years, had symptoms of commencing general paralysis; a third, aged four or five years, had a hydrocephalic head; and a fourth, which had lasted three years and a half, was otherwise favourable. In 6 of the remaining 11, the attacks were suppressed; and of the 5 others, 3 have had the intervals so much prolonged, as to afford hope of complete cure on continuance of the remedy; the remaining two were amended. The remedy was given for a period of from five or six to twelve months.

III *On Iodurelted Chloride of Mercury, and its Use in Acne Rosacea.* (Bull. Gén. de Thérap., Oct. 1855.)

This compound is formed either with one equivalent of iodine and two equivalents of calomel, or with single equivalents of the ingredients. For the preparation of the former compound, two equivalents of calomel are roughly powdered and introduced into a matrass, and gently heated and shaken till it begins to sublime; the iodine is then added in small portions, and the combination takes place noisily, without any sensible loss of iodine. To obtain the second compound, only one equivalent of calomel is used; the method of preparation being otherwise the same. The following formulæ are in use by M. Rochard:—*Pomade.* Take of iodurctted chloride of mercury, 0·75 grammes; fresh axunge, 60 grammes: mix carefully. *Pills.* Take of iodurctted chloride of mercury, 0·25 grammes; gum arabic, 1 gramme; bread-crumb, 9 grammes; orange-flower water, sufficient for twenty-five pills, of which one to three are to be taken daily. These medicines are made with the first compound; the second is formed into sticks, to act as a caustic. In the treatment of acne rosacea, the pomade is used by M. Rochard once a day, by way of friction on the diseased surface. This is repeated for two or three consecutive days, leaving the parts uncovered in the interval. The skin becomes excited under its influence, the circulation accelerated, and the heat augmented; an abundant discharge of simple serosity or puriform matter escapes, and, by exposure, becomes converted into crusts, which cover the points affected by the disease. These crusts after awhile fall, leaving the surface less red and less indurated. A new application produces a new discharge and new crusts, which, after their fall, leave a surface even more deeply altered than the first time. After several repetitions of this process, the skin resumes quite its natural aspect and normal texture. The amendment is announced by a diminished energy of the reaction; and the time at length arrives when no further discharge is produced, and this is the period of cure. When this topical treatment fails, the medicine may be cautiously given internally.

IV. *On the Administration of Quinine in Intermittent Fevers, &c.* By M. Briquet. (Bull. Gén. de Thérap., Oct. 1855.)

M. Briquet discusses the two modes of administering quinine—viz., in *small* doses, where the object is to obtain the greatest febrifuge effect possible from the smallest dose; and next, in large doses.

1. *Administration in Small Doses.*—He gives thus an acid solution of 25 or 30 centigrammes, with 1 centigramme of acetate of morphia, in 120 grammes of eau sucrée in five doses in five consecutive hours; and he has established the following points. 1st. That when the last dose has been administered at the moment of the invasion of the paroxysm, the latter has been very rarely modified. 2nd. That when an interval of from six to eight hours has elapsed between the last dose and the future paroxysm, this was arrested or favourably modified in half the cases, and that almost always the succeeding fit was prevented. 3rd. That when an interval of twelve hours had elapsed, the paroxysm was arrested in three-fourths of the cases, and the succeeding paroxysm constantly. 4th. That when an interval of fifteen hours was left, the future paroxysm never failed to be stopped, and the fever cut short completely. M. Briquet concludes from these that the sulphate should be commenced twenty hours before the fit, so that the specified quantity may be taken in the course of five hours, and that an interval of fifteen hours may be left between the last dose and the invasion of the paroxysm. This method, he says, is applicable to all simple cases, for we can always get eighteen to twenty hours between two paroxysms of quotidian fever, the paroxysms of which last, at the most, from four to five hours. In other types, tertian and quartan, there is no lack of time for using the quinine.

2. *Administration in Large Doses.*—In the fevers which are not simple—e. g., the double tertians, very severe quotidians, pernicious intermittents, and severe remittents, the time for acting with small doses is not obtainable, and recourse must be had to large doses. As it is known that, given in large doses, the quinine in solution begins to be absorbed almost instantly, and that it operates with all its intensity upon the nervous system in from half-an-hour to an hour after ingestion, it only remains to calculate the doses, so that the maximum of hyperstheniztion may be produced the greatest possible number of hours before the arrival of the paroxysm. In this way, with an interval of six hours between two paroxysms, there will be sufficient time—three hours to give the sulphate in, and three hours for it to act. Thus what is wanting in time is made up by quantity, which latter we can always control. He gives the following rules for avoiding the dangers and accidents which attend the use of large doses:—1st. To employ only a soluble preparation of quinine, and to give it in the form of solution. 2nd. To give it in divided doses, leaving an interval of an hour between each dose; and continuing it only during ten or twelve hours of the day, leaving the patient at rest during the remainder of the twenty-four hours. The quantities should consist of from 15 to 20 centigrammes an hour. 3rd. It is necessary gradually to increase the dose of each day in proportion to the resistance that the disease opposes. This increase may be from 40 to 50 centigrammes a-day; of course having regard to the tolerance of the patient. 4th. The sulphate of the alkaloids, and by preference the bisulphates, are the preparations to be employed. 5th. The treatment by large doses ought not to be employed with nervous impressible subjects, disposed to cerebral congestions, except with much attention, and very circumspectly. M. Briquet considers that the salts of morphia, united with the quinine, lessen the primitive excitant effect of this alkaloid, and notably increase its hyposthenic action: hence 10·2 centigrammes of acetate of morphia are advantageously added to 30 or 40 centigrammes of quinine. He says that bloodletting renders the nervous system more susceptible to the influence of quinine, and allows the quantity to be lessened without diminishing its febrifuge effect. In pernicious fevers, and acute articular rheumatism, where congestive phenomena exist, he considers bleeding a valuable adjuvant. He thinks that the value of emetics, as preparatory to the use of quinine, lies in their promoting the absorption of the latter.

V. *On Tar-Frictions—Absorption of the Tar.* By Dr. PETTERS. (Vierteljahrsch. für die Praktische Heilkunde, Band iii. 1855.)

Dr. Petters, in the instance of two individuals who were employing tar-ointment for the cure of psoriasis, has determined the fact of the absorption of the tar by the skin, and its excretion by the urine. He has specially determined the presence of carbolic acid, $C_{12} H_6 O_2$, in the urine of these patients, not free, but combined with soda; and considers that almost all the principles of the tar may pass, under such circumstances, through the organism, either in an unaltered or altered form.

VI. *On Coniin.* By Dr SCHROFF. (Wochenblatt der Zeitschr. der k. k. Gesellsch. der Aerzte zu Wien, No. 2 and seq. 1856.)

Twenty-seven experiments were made with coniin upon the human subject, three medical gentlemen having each submitted to nine. The doses given varied from 0·003 grammes to 0·085 grammes. The last and strongest dose which was taken corresponded to two drops of newly-prepared coniin taken out of a bottle opened for the first time. Dr. Schroff has found, by observations on rabbits, that exposure to the air weakens the operation of the alkaloid. This dose was dissolved in thirty drops of alcohol. The following account of the symptoms produced embraces those which resulted from the operation of smaller quantities. A very

sharp taste, strong burning in the mouth, sense of scraping in the throat, salivation; the epithelium of the tongue was removed in spots; the papillæ were strongly prominent, and the organ lost sensibility, and was as if paralysed. In about three minutes, the head and face became very warm, accompanied by a sense of fulness, weight, and pressure in the head (symptoms which were not produced by the smaller doses). These head symptoms reached a high degree of intensity; became associated with giddiness, inability to think or to fix the attention on one subject, with sleepiness, great general discomfort, and malaise (*Katzenjammer*), which, in a less degree, lasted till next day. The vision was indistinct, objects floating together, and the pupil was dilated; the hearing was obtuse, as if the ears were stopped with cotton; the sense of touch was indistinct, and there was a feeling of formication, and as if the skin were covered with fur; general weakness and prostration, so that the head was with difficulty kept erect; the upper extremities could only be moved with the exertion of much effort; and, on account of the weakness of the lower extremities, the walk was very uncertain and tottering. Even the next day the weakness of the extremities continued, slight trembling being induced by much movement. While going home, the muscular debility was especially great, the walk consisting rather of a throwing forward of the body, so as to bring the muscular action into as little use as possible. On stepping, and, when at home, on pulling off the boots, cramps in the calves of the legs occurred, as well as in other groups of muscles when they were called into action—as, for instance, in the balls of the thumbs when the thumbs were closely bent. This symptom was constantly observed in two of the experimenters when the dose was at least one drop. Under strong effort to move, pain in the muscles and legs occurred. Fresh air diminished the giddiness and fulness in the head, but in one of the experimenters, occasioned temporary pain in the course of the supra orbitalis and cutaneous malæ nerves Eructations, abdominal rumbling and distension, nausea, even efforts at vomiting, occurred in all the subjects, even after small doses; in one case, actual vomiting took place. Sometimes there was a tendency to diarrhœa. No effect was produced upon the urine In all the cases there was dampness of the ends of the fingers; and after large doses, the hands were absolutely moist. The countenance was sunken and pale; the hands were cold and blue. After the larger doses, the pulse commonly increased in frequency to the extent of a few beats, but subsequently it always lessened; yet this diminution did not bear that relation to the extent of the dose as where aconite was given. Respiration was often yawning, but otherwise no constant anomaly presented itself. The sleep was good, and mostly very sound.

VII. *On the Use of a new Solution of Iodine in various Skin Diseases.* By Dr. Max Richter. (Wochenblatt der Zeitschrift der k. k. Gesellsch. der Aerzte zu Wien, 1855. No 51.)

The solution is made thus:—Half an ounce of iodine is to be dissolved in an ounce of glycerine, and subsequently half an ounce of iodine is to be added, which completely dissolves in a few hours. In the experiments made with this solution, it was applied to the surface by means of a hair pencil; the part was then covered with gutta percha paper, fixed at the edges with strips of plaster, so as to prevent the volatilization of the iodine. This was removed after twenty four hours; and for a similar time, cold pledgets were applied. Burning pain, more or less intense, but rarely of more than two hours' duration, was produced The repetition of the painting depends on the appearance of the part and the amount of disease. The conclusions of the author are—1. That the iodine thus applied acts as a caustic; 2. That while it possesses considerable curative powers in respect of scrofulous and syphilitic affections, it is especially useful in lupus; 3. That the solution dissipates even deeply-seated tubercles of lupus, and may be applied for this purpose to the most tender surface without fear of eroding it; 4. That when the solution was

applied only to a part of a diseased surface, the remainder was, nevertheless, influenced; 5. That it is particularly serviceable to large and superficial sores; 6. That after a series of paintings, and when the sore was almost healed, the local pains greatly increased in intensity.

Albuminuria.—M. Mauthner,* in the albuminuria and dropsy after scarlatina in children, recommends the exclusive use of a milk or rice-milk diet. Under its influence, he says, an abundant urinary flux becomes established, which causes the disappearance of the dropsy; but should it not suffice for the cure, he would seek to modify the urinary secretion by the use of alkalies. He recommends urea to be given in such cases, in doses of one-third of a grain, mixed with sugar. When a dose of 6 or 8 grains is arrived at, he says, it occasions an abundant secretion of urine, and rapid disappearance of the dropsy.

Chilblains.—Professor Berthold† employs decoction of nut-galls as a bath, or applied by means of pledgets. The itching and burning disappear in two or three days, but in old cases the remedy must be continued longer. Oak bark (1 lb. to 2 lbs. of water) may be employed as a poultice. These remedies are not applicable to broken chilblains.

Convulsions—Chloroform Inhalations.—M. Marotte‡ relates the case of an infant, eleven months old, suffering from convulsions, with spasm of the glottis, during dentition. Chloroform was very cautiously administered, with immediate relief; and in half an hour sleep was procured, and kept up by occasional respiration of the vapour for two hours, and then natural sleep ensued Several relapses occurred—decreasing however in severity—each of which was treated in a similar manner; and altogether, 45 grammes of chloroform were expended.

Congenital Hernia.—M. Jobert§ relates four cases treated successfully by *iodine injections.* The plan was proposed by Velpeau, eighteen or twenty years ago. M. Jobert does not cut into the sac, after Velpeau's method, but merely punctures it, varying the operation a little, according as the sac is full or not of liquid.

Cephalalgia—Hydrochlorate of Morphia in Coffee.—M. Boileau‖ relates an obstinate case of cephalalgia, which he treated by hydrochlorate of morphia dissolved in strong infusion of coffee. The attacks occurred especially on any exposure of the head to cold air, and had resisted the operation of each of these remedies separately. It ceased almost immediately after taking them in combination, and by repeating the dose on each recurrence of the pain, the attacks became less frequent, and at length disappeared. M. Boileau says that he has found it successful in many other instances.

Chloroform.—Mr. Syme, in a clinical lecture,¶ directs attention to the importance of watching the respiration during the administration of chloroform vapour; regarding the indications afforded by the breathing as of greater value than those furnished by the pulse. On respiration becoming difficult, he directs that the mouth should be opened, and the tip of the tongue being seized with artery forceps, that it should be well drawn forwards.

Dr. Snow** has no hesitation in administering it, even in persons with fatty degeneration of the heart, believing that it is more likely to save life in such cases under operation than to destroy it, by preventing not only the straining and holding the breath, which would induce an over-distended state of the right cavities of the heart, but also the direct sedative operation of pain. He has given it to a number of persons with all the symptoms of fatty heart. His experience also

* Arch. Gén., April, 1855. † Pharm. Journ., Oct. 1855.
‡ Bull. Gén. de Thérap., April, 1855. § Rev. Méd.-Chir., April, 1855.
‖ Rev. Méd.-Chir., Feb. 1855. ¶ Lancet, Jan. 1855. ** Ibid., Oct. 20, 1855.

leads him to overlook the presence of chronic disease of the respiratory organs. He has given it without any ill effects in many cases where more or less paralysis remained from previous apoplexy, where patients have been reduced by various causes to a state of extreme debility, and in excessive exhaustion, in strangulated hernia, or compound fractures. He has also administered it to infants from ten days to three weeks old, and to one patient nearly ninety.

Chorea—Inhalation of Chloroform.—According to Dr. Géry,* chloroform inhalations have been used with advantage at the Hôp. des Enfans in severe cases where the violence of the movements have been beyond the control of opium or belladonna. It has been found at once to calm the movements and produce sleep, and in this way time has been gained for the employment of other remedies. On the first application of the vapour, the intensity of the movements is often greatly increased, but a calm succeeds as the inhalation is continued. Sound sleep thus induced lasts in children for ten or fifteen minutes, or even half an hour, and no ill effects have been observed to follow. The usual precautions, however, which are taken in the instance of adults, are necessary to be observed, such as ensuring that the stomach be empty, removing all obstacles to the respiratory movements, and watching the respiration and pulse, &c. The usual quantity administered has been ten to twenty grammes.

Dr. Bouchardt† relates a case of a girl in which severe chorea had lasted twenty-one days. She was subjected to the influence of chloroform twenty-seven times in fourteen days, at first twice, then three times, and lastly once a day, at the end of which time she was cured.

Chorea.—The *gymnastic treatment* of chorea has already been discussed in this journal, and we now subjoin the conclusions drawn from an extended experience of its use in the Hôpital des Enfans by M. Blache.‡ 1st. No other method of treatment applied to chorea has produced so large a number of cures as the gymnastic treatment, either alone or associated with sulphurous baths. 2nd. It may be employed in almost all cases, without being arrested by the various contra-indications which present themselves at each step in the use of the other remedies for the disease. 3rd. The cure is obtained in a mean number of days about equal to that which the sulphurous baths require, but it seems to be more lasting, and the diminution of the affection is exhibited from the first. 4th. At the same time that the disorder of the movements disappears, the general health of the children sensibly improves, and the patients depart not only cured of the chorea, but also of the anæmia which most frequently accompanies it. 5th. The gymnastic exercises, which might be regarded as perilous, especially in the instance of the children who are submitted to them, present no danger at all, and may be put in practice without inconvenience in all seasons, an advantage which the baths do not possess. 6th. It is important to divide the exercises into two categories—*a*, the *passive* exercises, which can alone be employed in that period of the affection where the will has no influence over the muscular powers; and *b*, the active exercises, which the children execute themselves, either with or without the aid of apparatus.

Diabetes.—M. Durand Fardel has observed that the use of the Vichy waters for twenty or thirty days is palliative in the greater number of cases, the sugar disappearing almost or entirely from the urine, the secretion becoming less abundant, the dryness of the tongue and skin lessening, and the general strength, digestion, and nutrition, becoming improved. The symptoms return, however, at a period more or less distant from the cessation of the treatment. The palliation, however, lasts longer than that obtained by mere dietetic restrictions, which moreover may be somewhat relaxed during the use of the waters.

* Bull. Gén. de Thérap. March, 1855. † Ibid., July, 1855.
‡ Rev. Méd.-Chir., Aug. 1855.

Ergot of Wheat.—Dr. Jobert* makes the following statements respecting this substance:—1. The medical and obstetrical property of this ergot is as incontestable as of ergot of rye, and its effects are as prompt, as direct, and as great. 2. Its hæmostatic action appears certain. Dr. Jobert has administered it several times against abundant discharges of blood, and immediately after labour it has almost constantly and fully succeeded. 3. In the dose of one or two grammes, according to urgency, in cases of uterine hæmorrhage, during any period of pregnancy, it has frequently succeeded in lessening, if not in completely arresting, the hæmorrhage; and this without appearing to produce any stimulant action on the uterus.

Epilepsy—Cotyledon Umbilicus.—Dr. Sieveking† narrates several cases in which this medicine was administered. No satisfactory conclusion can be drawn from them in respect to its efficacy.

Facial Neuralgia.—M. Lecointe‡ has employed chamomile, both in powder and concentrated infusion, in facial neuralgia, both periodic and non-periodic, with good results, even after other means of relief had failed; and believes it may in certain cases advantageously supplant the Peruvian bark. The dose, however, must not be less than four grammes; or, if given in infusion, the latter must be strong.

Ferrocyanide of Potassium and Urea.—Dr. V. Baud§ proposes this compound as a substitute for quinine in the treatment of some periodical diseases. He considers it applicable to those fevers, neuralgia, spasmodic diseases, and neuroses in which the intermittence is idiopathic, and not the result of marsh malaria. Its bitterness requires that it should be given in pills. M. Baud has usually given ten to fifteen pills of 15 centigrammes in the course of the day.

Gangrene of the Lung—Terebinthinate Inhalations.—Dr. Helm‖ narrates a case thus treated with success. The turpentine was poured upon hot water in Mudge's apparatus, and the vapour inhaled three times a day. The quantity of the expectoration began to diminish during the second week of the treatment. In the third week, the fœtidity lessened, and it lost its purulent aspect; and by the end of the fourth week the symptoms of the affection had almost disappeared. In the course of the next ten days all cough and expectoration ceased, and nothing morbid was any longer discoverable on examination of the chest.

Hæmorrhoidal Tumours—Actual Cautery.—M. Arthaud¶ directs attention to the advantages of the actual cautery to the treatment by ligature or excision, and relates some cases thus treated by M. Nélaton. He points out the precautions necessary to the operation. It is often followed by more or less severe vesical tenesmus, and sometimes by retention of urine. A tepid bath may not only relieve these symptoms, but calm the pain which succeeds the cautery. A light diet is recommended for the first five or six days, with a view to defer any action of the bowels. Towards the sixth day, however, in cachectic and debilitated subjects, an improved diet is indispensable, and tonics and steel must be prescribed. M. Nélaton has never observed any contraction of the rectum to result from this operation.

Hæmoptysis.—M. Aran** passes under review the several remedies which have been proposed to arrest this hæmorrhage. Venesection he considers not only to be useless, and its employment based upon no solid foundation, but dangerous, especially in phthisical subjects. He distributes the other remedies under three

* Gaz. des Hôpitaux, March, 1855. † Med. Times and Gaz., Dec. 2, 1854.
‡ Rev. Méd.-Chir., Jan. 1855. § L'Union Médicale, May, 1855.
‖ Wochenblatt der Zeitsch. der k. k. Gesellsch. der Aerzte zu Wien, Aug. 1855.
¶ Rev. Méd.-Chir., Feb. 1855. ** Bull. Gén. de Thérap., p. 193, 1855.

heads—hæmostatics proper, astringents, nauseants and emetics, and sedatives to the circulating system. Among the medicines of the first class, he recommends the use of oil of turpentine in debilitated and cachectic subjects, where the hæmorrhage is characterized by passiveness and atony. He regards common salt also, in large doses, as possessing incontestable efficacy. Among the astringents, he considers the acetate of lead to be adapted chiefly to the chronic and prolonged hæmoptyses, and also places great confidence in gallic acid. Admitting the efficacy of nauseants and emetics, ipecacuan, tartar emetic, and especially of veratrine, in the arrest of the hæmorrhage, he points out that precedence will always be accorded to less disagreeable remedies. The principal medicine to which he draws attention in the last class, is a combination of nitre and digitalis. In ordinary cases he gives, in four doses during the twenty-four hours, 30 centigrammes of digitalis and 1 gramme 50 centigrammes of nitre: when the hæmorrhage is more severe, he has carried the quantity of nitre to 2 grammes 50 centigrammes, and that of digitalis to 50 or 75 centigrammes, and in some very rare instances to 1 gramme 50 centigrammes; while the quantity of nitre has been raised to 4 grammes. In the extremely abundant hæmoptyses, depressing medicines must give place to turpentine and gallic acid, and even these must not be relied upon alone. He recommends the application of ligatures to the limbs and ice to the chest as means of arresting the bleeding temporarily, and of affording time for the internal medicines to complete the cure.

Hydatid Cyst of the Liver—Injection of Alcohol.—M. Richard* relates the case of a lady, aged forty, in which the cyst was thus treated. It was situated in the left lobe of the liver. It was evacuated by a fine trocar, and then 8 grammes of alcohol, of the strength of 36° (Beaume's aerometer), were injected and left in the cyst, the canula being quickly withdrawn. There was some acute pain for about five minutes. In two days' time there were some signs of reappearance of the tumour, followed by vomitings, loss of appetite, fever, and jaundice, which disappeared under appropriate treatment; the tumour lessened, and in three months' time had completely vanished.

Inhalation of the Vapour of Sal Ammoniac.—Dr. Giesler,† after inhaling the vapour to satisfy himself of its little irritating quality, administered it in several cases of chronic catarrh. The salt may be volatilized in a small Hessian crucible, heated by a spirit-lamp. The patient sitting before it, inhales the fumes, and the air of the apartment becomes also impregnated. Dr. Giesler has found these inhalations, employed two or three times a day, cure obstinate catarrhs in a few days, and has on no occasion found them useless. He recommends the vapour also in syndesmitis and strumous ophthalmia.

Iodide of Iron Pills.—M. Perreus‡ proposes the preparation of pills of the iodide according to the following formula:—Take of iodide, 1 gramme; powder of iron (not oxidized), 1 gramme; honey, 1 gramme; liquorice powder, 2 grammes. Rub together rapidly in an iron mortar the iodine and the powder of iron until they are completely mixed, then add the honey, and beat it till the mass becomes black and ceases to exhale an odour of iodine, then incorporate the liquorice powder with it, and divide rapidly into twenty-five pills. Silver them and preserve in a stoppered bottle, as they are slightly deliquescent. The presence of an excess of iron preserves the iodide for an indefinite period from the oxidating influence of the air.

Itch.—MM. Dusard and Pillon§ have employed with success the external application of a solution of 12 grammes of chloride of sulphur in 100 grammes of sulphuret of carbon. They pass lightly over the surface of the body a piece of lint

* Bull. Gén. de Thérap., May, 1855. † Henle und Pfeufer's Zeitschr., Band v. Heft 3.
‡ Bull. Gén. de Thérap., March, 1855. § L'Union Médicale, p. 439, Sept. 15, 1855.

soaked in the solution, an operation which does not occupy five minutes. The itching is said to cease at once, and the only subsequent treatment used after thirty-six hours is a simple bath, which is repeated every second day for a week. Complications are subsequently treated.

Juice of Meat.—Dr. Christison* writes in favour of the juice of meat prepared by Mr. Gillon of Leith. He finds it contains only six and a half per cent. of solids —beef tea containing in a pint scarcely a quarter of an ounce of solid matter. Gillon's meat-juice contains no fibrin, albumen, or gelatine, only osmazome and the salts and sapid principles of the meat. The value of beef tea in the treatment of protracted disease is familiar to all. Dr. Christison, considering how little really nutrient matter it contains, is disposed to think that it acts much as coffee appears to do, in hindering metamorphosis of tissue.

Lead Colic—Chloroform.—M. Aran,† after using chloroform in lead colic, both externally and internally, for four years, now repeats an opinion formerly expressed of its superiority over all other methods of treatment. At the same time he modifies some of his former statements. He regards the internal use of the medicine as the basis of the treatment, whilst he considers the application of it externally as only indispensable during the first days, and in the most severe cases. His observations have also taught him that it is impossible to lay down precisely the maximum dose, which must depend on the intensity of the pain, &c. It may be necessary to give as much as 100 or 300 drops (four to twelve grammes) in twenty-four hours, while 60 drops may suffice in slight cases. As the effects of chloroform rapidly pass away, the patient must be kept continually under its influence for a certain number of days by repeated small doses, given by the mouth or by enema. He applies the chloroform topically by dropping it on a fine and dry compress, to an amount varying with the degree of pain (e.g., two to four grammes), and after placing this upon the abdomen, it is covered with some dry compresses. It produces its effect in from one to five minutes. He gives the chloroform internally suspended in water by tragacanth. The lavement contains from 30 to 50 drops, similarly suspended. The topical application is rarely of any use beyond the second day. Reducing the dose, he continues the medicine in lavement, as a precaution, when the case has been severe, up to the eighth or twelfth day. He founds his recommendation on the results of 21 cases.

Mercurial Stomatitis—Chlorate of Potash.—M. Demarquay‡ relates 6 cases in which chlorate of potash succeeded rapidly in curing the stomatitis arising from the action of mercury. In one case, where the mercury had been used for a syphilitic affection, two grammes of this salt given for four days removed the stomatitis, and on resuming the mercurial treatment, and conjoining it with chlorate of potash, salivation did not reappear. The harmlessness of the salt is well known. M. Gustin took eight grammes, but did not find the urine increased; a sense of constriction in the mouth and some roughness of the gums were produced, and although the saliva was not sensibly diminished, yet it appeared to him more liquid than usual. He has established the fact, that the chlorate is in great part discharged unaltered by the urine. When the salivation has recently commenced, two-gramme doses suffice to check it; but when it is intense and fully established, the dose must be increased rapidly to five, ten, or fifteen grammes. He advises also the conjoined use of an astringent wash for the mouth.

Neuralgia.—A case is related which was under the care of M. van der Kiefft,§ in which neuralgia of the scrotum, which had resisted the internal use of tartar emetic, inhalation of chloroform, and the topical application of extract of belladonna and of chloroform, gave way under the internal use of chloroform in doses

* Monthly Journal of Medical Science, Jan. 1855.
† L'Union Médicale, Jan. 1855.
‡ Bull. Gen. de Thérap., May, 1855.
§ L'Union Méd., Feb. 1855.

of twenty drops every quarter of an hour. The pain lessened after the second dose, and disappeared after the third.

Ovarian Dropsy.—Mr. I. B. Brown* records an additional case, in which, after emptying the cyst, he injected five ounces of the tincture of iodine (Ph. Ed.) without producing pain, and apparently with the effect of arresting the progress of the disease.

Paraplegia—Phosphorus.—This substance, in the form of phosphuretted oil (gr. iv. of phosp. to ℥j. of olive oil), has been given by Dr. Hughes Bennett† in severe cases of paraplegia from diseases of the spinal cord, without improvement resulting in any one of them. The doses commenced with three drops a-day, and when he increased them to ten or fifteen drops, nausea and vomiting were induced. In a case of chronic myelitis, which took ten and fifteen drops, the breath smelt strongly of phosphorus, but was not phosphorescent at night. In another case, much phosphate was passed with the urine.

Pericarditis, with Effusion—Injection of Iodine.—A case is related by M. Aran‡ which demonstrates that the pericardium may be injected with iodine for the cure of effusions as well as other closed sacs. It is that of a young man, aged about twenty-three. The effusion resisted the application of the ordinary remedies, and unless he were relieved death was imminent. The respirations were 40, and the pulse 120—irregular and unequal. A puncture with a capillary trocar was made at the fifth interspace, two or three centimetres from the external limit of the dulness: 850 grammes of a transparent reddish serum were removed. The injection was then thrown in, consisting of fifty grammes of water, fifteen grammes of tincture of iodine, and one gramme of iodide of potassium. It produced no pain, and a few grammes were allowed to run out before the wound was closed. Re-accumulation having taken place, a second puncture and injection were had recourse to, the strength of the injected liquid being—water and tincture of iodine, of each fifty grammes; iodide of potassium, four grammes. Complete recovery resulted.

Photophobia—Tincture of Iodine.—M. van Holsbeek§ recommends as completely successful the external application of tincture of iodine, especially in that form which accompanies strumous ophthalmia and chronic granular conjunctivitis. He paints the orbicular and superciliary regions once or twice a-day, according to the duration and intensity of the symptom, and says that a single application usually suffices to remove the symptom in twenty-four hours.

Rheumatism—Veratrine.—Dr. Aliès‖ relates several cases of rheumatic affections in which he has rapidly effected a cure by the use of veratrine, in doses of five milligrammes every five or six hours.

Sciatica.—Dr. Blakiston¶ has treated eighty-three cases of the uncomplicated disease without a failure by the endermic application of morphia. A blister the size of a crown piece is used over the chief seat of pain, and a grain of acetate of morphia daily sprinkled on the denuded surface, which is prevented from healing, if necessary, by savine or cantharides cerate. Should the knee become painful, the same plan is adopted at the ham.

Skin Diseases.—Dr. Hughes Bennett** treats impetigo and eczema by assiduously keeping the parts moist with lint saturated with a solution of half a drachm of sub-carbonate of soda to one pint of water, covering this carefully with oil-silk, to prevent evaporation. Favus he treats with oil, so as to exclude the atmosphere

* Lancet, April 14, 1855. † Monthly Journal of Medical Science, Feb. 1855.
‡ L'Union Méd., Nov. 8, 1855. § Ibid., March, 1855. ‖ Ibid., Sept. 1855.
¶ Med. Times and Gaz., Jan. 1855. ** Monthly Journal of Med. Science, Jan. 1855.

and prevent the growth of the parasitic fungi, first removing the crusts by poulticing; an oil-skin cap must be constantly worn. He says that six weeks suffice for cure in young persons, if the treatment be conjoined with cod-liver oil, generous diet, and antiscrofulous remedies.

Small-pox—Tincture of Iodine.—M. Boinet adds his testimony* to the efficacy of tincture of iodine in causing the abortion of the variolous pustules. He adds a number of formulæ for its use also in vaginitis, blennorrhagia, inflammations and ulcerations of the mouth and throat, and in mercurial ptyalism, erysipelas, and ophthalmia. In all these instances he combines it with tannic acid in various proportions.

Small-pox.—Dr. Wallace† has used a solution of gutta percha in chloroform as a topical application to prevent pitting in twenty cases, four being confluent, seven semi-confluent, seven discrete and moderate, and two with the eruptions discrete but copious. In all it was painted on immediately before complete maturation. Scarcely any pitting was observed when the patients were dismissed, except in two or three instances, where it was moderate. Some months after discharge, however, such of the cases as he then had an opportunity of seeing presented the marks much more distinctly. He insists, that in estimating the comparative methods of preventing pitting, regard should be had to the appearances a long time after treatment as well as on dismissal.

Spermatorrhœa—Belladonna.—In those cases which are connected with chronic inflammation of the urethra, good results followed cauterisation of the prostatic portion of that canal, but M. Trousseau‡ believes that cases arise in which the disease is purely spasmodic, and resembles certain forms of incontinence of urine in children. For these cases he advises the use of belladonna internally, of friction of belladonna ointment upon the perinæum, and of belladonna suppositories. Where an erotic condition is conjoined, he thinks the best sedative is the application of bags of hot sand to the perinæum for a few minutes morning and evening. Lupulin, in doses of fifty centigrammes a-day, may be tried; or bromide of potassium, on which M. Trousseau places reliance, in doses of one or two grammes a-day.

Suppressed Secretion of Milk—Faradization.—A case is recorded by M. Aubert of Macon,§ where, in consequence of suspension of suckling from the infant being attacked with pneumonia, the secretion of milk had gradually subsided. Fifteen days after the child had been first attacked, suction having failed to restore the secretion, Faradization (the application of the induced electric current) of the breasts, by means of the apparatus of Dr Duchenne, was commenced, the force of the current and vibrations being gradually increased, but not to the extent of producing pain or contraction of the pectoral muscles. After a few minutes, the right breast increased in size. The operation was repeated daily, and on the third occasion the suction of the infant subsequently induced a slight "draught." The child had drawn a little milk since the second application of the remedy. On the fifth day it is reported that "the draught" had been twice complete, and the lactation was considered re-established.

Tetanus—Chloroform Inhalations.—Dr. Sprengler relates a case of traumatic tetanus,‖ in which the free and frequent inhalation of chloroform, but short of complete anæsthesia, was had recourse to. The disease lasted from the 13th of July to the evening of the 17th, and the diaphragm and heart remained unaffected till the last hours of life. The author thinks that if by this means the course of tetanus can be rendered more chronic, the prospect of cure may be improved.

* Rev. Méd.-Chir., Feb. 1855. † Glasgow Med. Journal, 1855.
‡ Rev. Méd.-Chir., March, 1855 § L'Union Méd., Sept.
‖ Henle und Pfeufer's Zeitschrift, Band v. Heft 3.

Uva Ursi an Ecbolic.—Mr. Harris* has found a strong decoction of uva ursi, during labour, in cases where ergot is applicable, produce vigorous pains, which soon caused expulsion of the child and placenta. The contractions induced are less violent than those from ergot.

Yellow Fever—Turpentine.—Sixty cases on board H.M.S. *Medea* were treated with turpentine by Mr. Laird.† Of these only four died. The usual dose was fifteen minims every three or four hours, with a little nitric ether and camphor mixture, continued till a remission was apparent. He regards its action as eliminative by the skin and kidneys.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By Edward H. Sieveking, M.D.
FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, ETC. ETC.

I. *On the Black Colour of the Tongue, apart from Febrile Conditions.* By M. Bertrand de St. Germain. (L'Union Médicale, Dec. 8, 1855.)

M. de St. Germain states that he has, in twelve years, four times met with a spot of an oval shape and intense black colour on the middle line of tongue, gradually spreading over the entire organ. The discoloration remained stationary for about ten days, and then gradually disappeared from the circumference to the centre, the changes resembling those of an ecchymosis undergoing resolution. The entire duration of the phenomenon was from forty to sixty days. The patients complained of no local symptoms, but those of dryness of the mouth. The four cases were: 1. A girl, aged thirteen, whose increasing emaciation and paraplegia denoted a serious lesion of the nervous centres. 2. An asthmatic lady, aged seventy, whose health was not more than usually impaired. 3. An old man, in fair health. And 4. A girl, aged eleven, convalescent of typhoid fever. The author regards it as an accidental formation of pigmentary matter, not connected with hæmorrhage. He does not advert to the question whether his patients were or were not taking steel medicines at the time.

II. *Acute Fibroid Cancer of Thyroid Gland. Death from Compression of the Right Vagus.* By E. Monvard Martin, Physician to the Hôpital St. Antoine. (L'Union Médicale, Feb. 23, 1856.)

Baumgartner, aged thirty-six, a female servant, in good bodily health, but subject to some intellectual obliquity, left the service of her master, a medical man, on a visit to her native country, and returned after three months' absence. A few days after her return (10th of November) she found her neck enlarged about the middle, and the enlargement increased so rapidly, that when admitted into the Hôpital St. Antoine, it had already acquired the size of half a hen's egg. It lay in front of the larynx and trachea, more to the right than the left, reaching above to the upper border of the thyroid cartilage. The larynx was strongly pushed over to the left, so that when the head was held erect, it lay exactly under the angle of the lower jaw. There was no pain or tenderness, during rest respiration was not laboured, but became very noisy as soon as the head was raised, or the patient moved actively about. There was no dysphagia. The treatment consisted in the administration of iodide of potassium and calomel; but the tumour rapidly enlarged in every direction, causing increasing dyspnœa, loss of voice,

* Virginian Med. Journal. † Medical Times and Gazette, April, 1855.

dysphagia, and, on the thirteenth day after admission, death ensued, without any material change in the symptoms. The affection was regarded by Dr. Martin and his colleague, M. Richet, as an inflammatory affection of the thyroid. The autopsy showed the tumour to consist of two distinct lobes which laterally extended as far as the vertebral column, and from the central portion a shoot descended to 4 centimètres (1·5 English inch) below the upper border of the sternum. The larynx was completely carried over to the left of the neck, but the trachea was curved still further to the left, then returning to the sternum, assumed a vertical position. Nowhere did it show any contraction. The œsophagus followed the same direction as the trachea, being enveloped equally by the growth. The left vagus, common carotid, and jugular vein, were free, and lay at the back and side of the tumour. The right phrenic nerve passed over the tumour, as did the right internal jugular vein; but the right vagus, which was separated about half an inch from the vein, was involved in the tumour, hypertrophied, and reddened. The right carotid was equally imbedded in the growth. At the inferior part of the tumour, a large vein presented a perfectly white appearance, from containing a firm clot of a semi-transparent opaline character. The tumour, on being excised, resisted the scalpel, presented a mottled white surface; it was divided into compartments by fibrous tissue, and on scraping the cut surface, the characteristic juice of cancer was obtained. The microscopic appearances, which, however, are not detailed, presented by the tumour and the clot in the vein were identical, and convinced the author that he had to do with a fibrous cancer of the thyroid body. All the other organs were healthy.

The author observes that the case is an exceptional one, on account of the rapid growth of the fibrous form of cancer. He considers that death resulted from the compression of the right vagus, since the calibre of the trachea was not interfered with. The patient died of defective hæmatosis, in spite of the admission of air into the pulmonary vesicles. For the same reason, he congratulates himself upon not having performed tracheotomy, which could not have influenced the morbid condition of the right vagus.

III. *On the Treatment of Pneumonia and Pleurisy.* By Dr. NIEMEYER, Professor of Clinical Medicine in Greifswalde. (Prager Vierteljahrsschrift, 1855. Band IV. p. 121.)

Professor Niemeyer is much opposed to the employment of general venesection in pneumonia and pleurisy, and only uses it exceptionally with a view to prevent impending suffocation, and to facilitate the reflux of the blood from the brain, but not for the purpose of arresting the inflammation. He agrees with the observation of Dieck, that the convalescence is more rapid in those cases that have been treated without than those which have been treated with venesection; and he explains the fact by the increase of fibrin, and diminution in the amount of red corpuscles, induced by the venesection.

The treatment adopted by Professor Niemeyer consists in the application of compresses wrung out in cold water over the affected part of the thorax, and their renewal as often as they become warm. The great relief experienced by the patient is a sufficient guarantee that the repetition of the application will be carefully attended to. The only internal remedy employed was nitre, in doses of two drachms in the course of twenty-four hours. Although employed at different ages, and in various forms of the disease, no metastasis or other evil consequences have ever been noticed by the author. He has seen persons attacked with very tumultuous symptoms, enabled by this treatment to return to their occupation on the seventh day after seizure. Professor Niemeyer recommends an early exhibition of steel in the convalescence from the diseases under consideration.

IV. *A Case proving the Advantage of Iodine Injections in Empyæma.* By M. BOINET. (L'Union Médicale, Dec. 22, 1855.)

In the April number of the 'Medico-Chirurgical Review' we gave a case proving the advantage of iodine injections into the pleura in the treatment of hydatids in the thoracic cavity. The present case, that of a lady, aged thirty-four, affected with pleurisy, occurred in the practice of M. Trousseau. Thoracentesis was performed in January, 1855, and two and a half litres (about five pints) of serum evacuated from the right pleura. The pleura refilled, and on the repetition of thoracentesis the fluid was found to have become purulent. It was now (in April, 1855) entirely withdrawn by M. Boinet, the pleural cavity washed out with warm water, and an injection of equal parts (fifty grammes of each) of water and tincture of iodine, with two grammes of iodide of potassium, thrown in. The injection was withdrawn after five minutes, and repeated at intervals of at first two or three days, and then at longer periods. The canula was left permanently in the thorax, the orifice being well closed after each operation. No bad symptom occurred; after a fortnight the patient was able to leave her bed, and a week after to walk about. In June she went about Paris, and in July she was well enough to undertake a journey. The right side of the thorax has sunk in, but the corresponding lung has in part recovered its functions.

V. *A Paper on the Effects of Lead on the Heart.* By T. W. CORSON., M.D. (New York Journal of Medicine, March, 1856.)

In the course of a general inquiry into the relation of an altered impulse of the heart to various maladies, Dr. Corson has observed that house-painters and others, with either paralysis or great muscular debility from lead-poisoning, had uniformly a more or less weakened impulse of the heart, and generally on going upstairs complained of some faintness and cardiac distress. In the earlier stage, where lead colic only existed, it seemed as if the heart had as yet escaped, and these symptoms were absent. The practical advantage of a correct diagnosis of heart-symptoms depending upon such a cause, is that we can at once hold out great probability of a complete cure, by means of those agents which counteract the lead-poisoning and eliminate the metallic salts from the system. Dr. Corson gives the details of ten cases of well-marked lead-poisoning, all characterized by the Burtonian streak, in which, in addition to more or less paralysis of the extremities, much alarm was excited by the great feebleness of the heart, with palpitation, consequent fainting, weak and soft pulse. The uniform effect of the treatment employed for the metallic poisoning, by iodide of potassium and nux vomica, aided by occasional tonics, baths, laxatives, and electricity, was to relieve or remove the cardiac symptoms.

Dr. Corson observes that the debilitating effects of lead most commonly occur in hearts previously sound; but that they sometimes, as in two of his cases, complicate existing organic cardiac disease from rheumatism or other causes. He is also of opinion that while the agencies or causes of lead-poisoning are very numerous and often obscure, the slighter cases, supposed to be ordinary dyspepsia, constipation, or bilious colic, are frequently undetected. Dr. Corson regards the action of lead upon the heart as analogous to that of digitalis; oil of tobacco, upas antiar, woorara, tending specially to paralyse the organ. His treatment is that adopted by many practitioners, more especially since the appearance of M. Melsen's memoirs; and consists in the exhibition of iodide of potassium with a view to the elimination of the lead from the system, followed by strychnia or nux vomica, with a view to rouse the enfeebled action of the nervous system.

VI. *Elimination of Urea by the Gastric Mucous Membrane in a Case of Fatal Traumatic Hæmaturia.* By Dr. M'DOWEL. (Dublin Hospital Gazette, March 15, 1856.)

A gentleman, aged thirty-eight, received, in December, 1855, a severe injury in the right lumbar region by falling down stairs, followed by uncontrollable hæmaturia. A few days later vomiting supervened, without symptoms of gastritis; the tongue was natural, and the epigastrium free from tenderness. "It was observed that during these attacks of vomiting the amount of blood discharged from the kidney seemed to be greater, the urinous smell being faintly perceptible, whilst at other times it was strongly marked from the presence of ammonia. Medicine seemed to have little influence in relieving these later attacks, each of which lasted for about two days, and seemed to subside of its own accord."

The erethism of the stomach increased, and continued until death. Everything was rejected, and medicine was wholly useless. The mental faculties were unimpaired to the last. Owing to this circumstance Dr. M'Dowel suspected that the mucous membrane of the stomach was vicariously secreting urea; and on taking the vomited matter to Dr. E. Davy his suspicions were confirmed, for this gentleman found the vomited matter to contain both urea and ammonia, the amount of nitrogen in both being equal to one-fifth part of a grain of urea in the fluid ounce. The renal secretion was alkaline, and contained 1·23 grains of urea to the fluid ounce. Dr. Davy makes the following remarks on the analysis:

"These results show that the quantity of nitrogen removed from the system by the kidneys is extremely small (if we except that contained in the blood occurring in the urine). They also show that the stomach assists the kidneys in removing nitrogen out of the system, which function those organs, in their present diseased state, are unable sufficiently to perform; and though the fluid vomited contains in the same bulk a much less amount of nitrogen than that in the urine, yet if the quantity of vomited fluid is much more copious, the stomach may, in the twenty-four hours, eliminate a greater amount of nitrogen from the system. The large quantity of albumen found would indicate that a considerable amount of blood was present in the urine, as it is most probable the albumen was furnished from the serum of the blood.

"The quantity of oily matter present in the urine was very remarkable; such matter is said to occur in the urine in diseases in which there is a very rapid loss of substance or force, which I believe is the case with your patient."

VII. *Fatty Degeneration of the Liver and other Organs considered as the Chief Cause of the High Rate of Mortality among the European Troops of the H. E. I. C. in Bengal.* By C. N. MACNAMARA, Esq., Assistant-Surgeon H. E. I. C. S. (Indian Annals of Medical Science, October, 1855, p. 169.)

The title of Mr. Macnamara's paper indicates in part his object, which he also expresses thus: "What I wish to prove is, that it is the mode of living of Europeans in India, and not the climate, which kills them." "Perhaps," he shortly afterwards adds, "one of the best proofs I could bring forward to show that such is the case, is the fact that, taking the whole strength of the officers and men of the First European Bengal Fusiliers during the last seven years, it will be found that the mortality among the former amounts to 11 per cent. for that time, and that the mortality among the latter amounts to nearly 80 per cent., thus proving what I have repeatedly been told by the officers of the corps,—that the whole of the fighting men in the regiment are changed about once in every ten years." The average duration of life of the privates of the same corps is only twenty-seven or twenty-eight years, which is at least seventeen less than that of the journeyman bakers of London, whose habits and mode of living are by no means enviable. In order to ascertain the causes of the remarkable mortality just adverted

to, the author institutes a comparison between the dietary of the various corps. In her Majesty's navy, each man is allowed from 31 to 35½ ounces of dry nutritious food, 26 being vegetable, the rest animal; in addition to sugar and cocoa. In the army, a man is allowed one pound of bread and three quarters of a pound of meat. The Company's allowance is to each man per diem, one pound of bread, one of beef, a quarter of a pound of rice, and half a pound of vegetables. This would make 44 ounces, in addition to which, Mr. Macnamara states that the men generally consume meat, vegetables, and rice, so as to make the day's allowance altogether amount to 76 ounces. The author argues that the Indian soldier cannot possibly require more, or as much food as the sailor, whose body is exposed to greater wear and tear, and who requires so much more carbonaceous food to keep up the due temperature. The author further reasons that as the hydrocarbons taken into the body in India cannot be all eliminated by the lungs, and they are not deposited in the form of subcutaneous fat, we may expect to find them clogging the liver, as the organ, physiologically, complementary to the lungs. That there is a great excess of oily matter in the circulation is shown, according to Mr. Macnamara's observation, by the microscope. In a former number, we had occasion to advert to Dr. Forbes Watson's observation of a fatty degeneration of the blood-globules occurring in certain diseased conditions in India; it is not improbable that, in other cases of fatty degeneration, the oily deposit will show itself in and external to the cell growth of the part. Mr. Macnamara does not supply us with copious statistics in proof of his position, but reports that in twenty-four post-mortems he only found one in which the liver did not present a state of greater or less fatty degeneration; the exceptional instance was accounted for by the fact that the man had been in the hospital for some time previous to his death, and that his diet had been much restricted.

As a secondary affection, the author finds fatty urine, resulting from the oily blood causing fatty degeneration of the renal epithelium and secretion of oil with the urine. In many instances, Mr. Macnamara has detected large quantities of cystine in the urine, which he regards as resulting from the decomposition of the urea and uric acid, owing to the excess of sulphur in the system, caused by the impeded excretion from the liver.

It is not surprising that, with so large a surplus of oily matter in the circulating fluid, the central organ of the circulation should also manifest a considerable fatty degeneration. In all the post-mortems but one, in which the liver was healthy, there was a large accumulation of fat upon and among the muscular tissue, and in many, the sarcolemma was replaced by [illegible] globules. A fatty degeneration of the large vessels, arterial and [illegible]ous, was [illegible]o strongly marked. In three of the twenty-four post-mortems, death was the result of rupture of vessels in that condition; in one, the vena cava was ruptured just at its junction with the auricle; in the other two cases, the aorta had given way, in one case, within, in the other, just outside, the pericardium. The walls of the vessels were found infiltrated with fat, so as to render them incapable of resisting the impulse of the blood.

The author concludes his interesting and valuable communication by dwelling on the necessity of counteracting the fearful mortality and its causes adverted to, by a corrected dietary. "Nearly half the men in the regiment," he observes, "are Irish or Scotch; most of them, before they left their native shores, had never tasted fresh meat; they had also, from the force of circumstances, been obliged to live a sober life." If such men, on joining the regiment at Dinapore, take to eating three pounds and upwards of solid food, under circumstances in which there is less necessity than before for so large an increase, they indeed come under the category of the glutton, and may be classed with those unfortunate birds who, when forced into similar circumstances, supply us with the *paté de foie d'oie*. The commissariat is out of our department; but as we have shown the pathological view of the question, it is to be hoped that it will not be lost sight of by those whose special office it is to attend to the regimen and dietary of the troops in question.

VIII. *On Paralysis from Muscular Atrophy.* By M. CRUVEILHIER. (Archives Générales de Médecine, Jan. 1856.)

In this paper, Cruveilhier claims the priority of the observation of muscular paralysis* apart from any lesion of the nervous centres. He gives a brief account of the first cases which came under his notice, in 1832 and 1848, and describes the surprise he felt when after, in the first instance, having diagnosed disease of the spinal cord, he found no trace of any affection of that part. He details two later cases, which satisfied him that the anterior roots of the spinal nerves exhibited a degree of atrophy closely corresponding to the amount of muscular degeneration upon which the paralysis depends. We must content ourselves with directing our readers' attention to these cases,† and giving an abridged summary of the author's views on the subject. They are as follows:

1. There is a species of paralysis, partial or general, which gradually affects the voluntary muscles, without involving the general or special sensibility, the intellectual or emotional powers, or any function of nutrition, except that bearing upon the muscles.

2. This muscular paralysis is the result of the progressive atrophy of the anterior roots of the spinal nerves, together with the progressive atrophy of the muscles; the posterior roots of the nerves, the spinal cord, and the encephalon remaining sound.

3. This form of paralysis is analogous to that resulting from section of a nerve.

4. This form of paralysis fully confirms the doctrine of Sir C. Bell, relative to the functions of the anterior and posterior roots of the nerves.

5. These observations establish the fact, previously not suspected, that the anterior roots of the nerves exercise a definite influence over nutrition.

6. These observations establish an independence of the anterior spinal roots, from the antero-lateral tracts of the cord, in which not the slightest disorganization was traceable. From this the author concludes,

7. That the anterior roots of the spinal nerves do not spring from the anterior-lateral tracts of the cord, but necessarily from the central grey matter.

Those interested in the whole history of symptomatology and therapeutics of progressive muscular atrophy, we would refer to a very complete memoir on the subject in all its bearings, by Dr. Adolph Wachsmuth.‡ He does full justice both to Cruveilhier and Dr. Meryon's claims, but points out that the 'Medical Gazette' of 1831§ contains an article by Dr. Darwall, in which that writer describes several instances of muscular atrophy with paralysis of the upper extremities, but attributes them to previous disease of the peripheral nerves. Dr. Wachsmuth has collected altogether sixty cases of the affection.

IX. *Communications from the Department for Spasmodic Affections in the Charité.* By Dr. LUDWIG MEYER. (Annalen des Charité Krankenhauses. Sechster Jahrgang, Heft 2, p. 1.)

In spite of the careful investigations to which the various forms of nervous affections have been submitted, and the rigid classifications laid down in standard works, the diagnosis between them is by no means always easy, and the transition from one to the other often imperceptible. Thus, hysteria has been commonly regarded as essentially dependent upon derangement of the female sexual system, and as essentially distinct from epilepsy; but, although the difference in well-

* It is but just to Dr. Edward Meryon (who published some interesting cases of granular and fatty degeneration of the voluntary muscles in the Medico-Chirurgical Transactions for 1852, p. 73) to state, that he was the first to observe the disease in England, and apparently without any knowledge of Cruveilhier's discovery.

† See, also, British and Foreign Medico-Chirurgical Review, Oct. 1855, p. 532.

‡ Henle und Pfeufer's Zeitschrift, Band vii. Hefte 1 & 2, pp. 1—128.

§ Cases of a Peculiar Species of Paralysis, By John Darwall, M.D.: Medical Gazette, vol. vii, p. 201.

marked symptoms is sufficient to justify a distinct classification, we see hysteria pass into epilepsy, and we frequently meet with undefined seizures of a nervous character, in which it is difficult, if not impossible, to determine the class they belong to. Brachet* has repeatedly met with cases of a transition of hysteria into epilepsy; and Thomas Willis already mentions similar instances.† Dr. Meyer gives one of two cases which he observed, in which symptoms of an hysterical character were converted into epileptic paroxysms. No observant practitioner can fail to have seen cases in which symptoms long unappreciated, or not properly regarded, were after the outbreak of an epileptic paroxysm at once interpreted as its precursor, or as what the French call the *petit mal*. The author also adverts to the question of the relation of insanity to spasmodic affections, and more particularly to the predominant influence of moral or emotional causes in the production of epilepsy.

Dr. Meyer's observations with regard to the efficacy of remedial agents are very unsatisfactory; he says, that although all the lauded specifics against epilepsy have been largely tried in the Charité, he has only met with one case in which there was definite proof of medicinal benefit. In this case it was produced by large doses of disulphate of quina.

X. *Observations on the Temperature of the Body in Intermittent Fever.* By Dr. S. A. Michael. (Archiv fur Physiologische Heilkunde von Vierordt. Jahrg., 1856. Heft i. p. 39.)

This paper contains two series of observations on the temperature of the surface in persons suffering under intermittent fever. The first contains eleven cases of intermittents of different types, in which the observations were taken every hour, or at least frequently, in the course of the day; the second comprises three cases, in which the observations were made during the paroxysms themselves, and generally every five minutes. The state of the pulse and the respiration were generally noted at the same time. The number of thermometric observations amounted to about 260. They were taken by placing the thermometer in the armpit. The following are the general conclusions arrived at by Dr. Michael:—

1. An increase of temperature from the normal state or the lowest apyretic condition, at first slow, shortly before or at the commencement of the rigor, rapidly and continuously advances, and then attains its maximum by successive intermittent advances.

2. The temperature remains at its maximum height for a period never exceeding two hours, but generally much less.

3. The diminution always takes place less rapidly than the elevation. It is affected in a graduated manner, each depression of the temperature being followed by an arrest.

4. The sensations of the patient are not in the ratio of the changes of temperature. The temperature is above that of the normal condition, both at the commencement of the rigor and at the termination of the sweating stage. The maximum temperatures occur either during the hot stage, towards the termination of the cold, or at the commencement of the sweating stage. These remarks apply to the various forms of intermittent fever.

5. In most of the cases, the maximum lay between 32° and 33° R. (104°—106¼° F.) The highest maximum was 33¼° R.

6. The duration of the paroxysms varies considerably in the cases presenting a tertian type. The limits are sixteen and thirty-two hours; in the quotidian forms they are nine and eighteen hours.

* Traité de l'Hystérie. Par T. L. Brachet. Ouvr. Cour. p. 376. 1847.
† Pathol. Cereb. et Nervor. Gener. Specimen. 1667.

7. The duration of the period of increase is always shorter than the period of decrease in the quotidian forms; in the tertian it is sometimes shorter, sometimes longer.

8. During the free intervals, the temperature generally falls below the normal temperature, still, the instances—especially of the quotidian fevers—are not rare in which it is at least several degrees (Réaumur) above the normal temperature.

9. After the exhibition of sulphate of chinidine (the salt commonly employed in Dr. Michael's cases) in doses of from ten to fifteen grains, there is either no recurrence of an increase of temperature, or a single increase of almost the same intensity, but with less violent subjective symptoms; or again, the temperature rises, though to a lower degree, and the symptoms are scarcely perceptible; or, finally, there is a feeble increase of temperature without any subjective symptoms. Only one case occurred in which there were two increases of temperature.

10. During convalescence, the temperature is generally under the normal elevation, but may occasionally rise a few tenths of a degree above it. At times there are evening exacerbations or evening remissions, or it is the same morning and evening.

XI. *Idiopathic Gangrene of the Four Extremities, resembling Gangrenous Ergotism.* By Bernard Henry, M.D. (Hollingworth's Medical Examiner, March, 1856.)

Our readers may remember the details of an interesting case of gangrene attributed by the author to ergotism, which was published in this Review by Mr. Camps.* The instance given by Dr. Henry closely resembles the former case, but was more severe, and did not in any way appear connected with ergotic poisoning. It occurred in a sempstress, aged forty-two, a widow, but of abandoned habits, who was admitted into the Philadelphia Hospital on November 22nd, 1855. A persistent attack of diarrhœa during the preceding summer, with habits of intemperance and syphilitic affections, had reduced her much. A fortnight before admission, she first felt a stinging sensation in her hands and feet, which became more painful by scratching, assumed a dusky red colour, which increased in lividity up to the period of her admission. She was at first supposed to be labouring under purpura. Dr. Henry says that—

"When she first came under his care her countenance was icterode, with an anxious expression, the conjunctivæ yellow, eyelids puffy, the intellect remarkably clear, the hands and fore-arms, for about a third of their length, of a leaden hue, deepening off to the fingers; these were flexed on the hand, black in colour, and dry and shrivelled in appearance. The feet and lower third of the legs were in a similar state. The tip of the nose and the skin over both patellæ were of a dusky colour, as though brushed over with bronze paint. The tongue was not much coated, but was marked with two longitudinal reddish-brown stains. The pulse was eighty, quick and small.

"The affected extremities were icy cold to the touch, and sensibility was so destroyed that the prick of a pin inserted in them was not felt. Sensibility above the line of discoloration was acute. Movement gave much pain; the weight and warmth of the bed clothes could not be borne; the cold air was more agreeable. The cartilages of the ears showed a commencing similar condition. The bowels at this time were constipated, and the urinary secretion small in quantity.

"She was ordered milkpunch, opium, and nutritious diet. The legs were enveloped in cotton wadding; this was afterwards removed at her own request.

"November 24th. The discoloration of the extremities has extended up an inch higher; no line of demarcation is perceptible, the livid hue shading off into the normal colour. The pulse remains small, and the urine scanty. Ol. terebinth. gtts. x. every fourth hour were added to the treatment.

* See British and Foreign Medico-Chirurgical Review, July, 1855, p. 196.

"November 26th. Vesications, filled with a dark-red serous fluid, made their appearance at the edges of the discoloured parts. A specimen of urine was obtained, passed before breakfast. It was high-coloured, of a reddish tinge, sp. grav. 1010, reaction alkaline, and exhibits mucus and purpurine.

"The case progressed without much alteration in the general symptoms. The lines of demarcation between the sound and affected parts by degrees became more distinct."

The line of demarcation gradually became sufficiently distinct on the arms to justify their removal. On the 26th December—

"The right hand was removed by sawing through the exposed bones. The granulations were dissected up, to make as fair a stump as possible under the circumstances. No vessels were taken up, but the cut extremities of the bone bled freely. She experienced very little pain, and no inconvenience, from the operation."

Two days after, the left arm was taken off, under similar circumstances.

"Very little change took place during the following week; the stumps showed a disposition to heal well; and the line of separation between the sound and gangrenous parts of the lower extremities was so marked as to justify their removal by amputation, did her strength permit. Her appetite, however, continued bad; she rejected nearly all food, and sleep was procured only by means of large doses of opium.

"Tonics and stimulants were administered whenever they could be retained."

But she sank gradually, and expired comatose on the 11th January.

"*Autopsy thirty hours after death.*—Present, Drs. West, Hunt, Kenderdine, Hall, and Henry. Emaciation not very great. On opening the thorax and abdomen, the viscera were found remarkably dry—scarcely any moisture; very little blood in cutting across the large arterial trunks. The whole venous system appeared engorged with black, thick blood. The lungs were perfectly healthy. Adhesions of the right pleura. On opening the pericardium, no fluid was found. The heart was rather small, the coronary veins engorged, as was the whole venous system. The tissue, also, of this organ was more soft than natural, with a tendency to fatty degeneration, and slight fatty deposits in the valves. The pulmonary artery and valves were natural in their structure, but contained a venous clot. The auriculo-ventricular opening was contracted, so as with difficulty to admit the finger. The valves of the aorta were normal; a coagulum was found in the descending aorta. The brachial and femoral arteries were dissected up and examined; they presented no unnatural appearance, but were found adherent to the bone, and closed at the line of demarcation.

"On opening the abdomen, the liver presented itself fatty and very much enlarged. There appeared to be commencing cirrhosis; there was resistance to the knife on cutting through the lobuli of that viscus. The other organs presented nothing remarkable."

Dr. Henry is of opinion that, in this case, there was no ground for suspecting ergotism, as the patient had always had abundant and excellent food, and as the harvest of the past year had been remarkably good, and sufficiently plentiful to place good food in the reach of the poorest classes. He attributes the gangrene to a diseased crasis of the blood, unconnected with any morbid condition of the arterial coats, and dependent upon the dissolute habits of the patient, which had impaired her constitution. It is not, however, to be overlooked that she is said to have eaten rye bread shortly before her attack, though "the amount is described as small; nor was there any evidence of its being of bad quality; and no other individuals of the family in which she resided, or living in her neighbourhood, were similarly affected."

XII. *Analysis of the Cadaveric Inspections made in the Pathologico-Anatomical Institution at Prague from May* 1, 1854, *to March* 30, 1855. By Dr. Arthur Willigk. (Prager Vierteljahrsschrift für praktische Heilkunde, xiii. Jahrgang, 1856. p. 1.)

This analysis is based upon the cadaveric examination of 1146 subjects (out of a total of 1806 deaths); of which 516, or 45·0 per cent., were males, and 630, or 54·9 per cent., females; but it also includes the results obtained by the total cadaveric inspections made by the author in previous years, amounting in all to nearly 5000 cases. We are unable to go through the different items seriatim or in detail, but shall select one or two which may appear to present points of special interest to our readers.

One quarter of all deaths (452) were due to tuberculosis; 254 were males, and 198 females. Among the former the greatest mortality occurred between the ages of thirty and forty; among the latter the greatest mortality was between twenty and thirty. The mortality from this cause was 9 2 per cent. in March, April, and May respectively; 6·3 per cent. for each of the three succeeding months; 5·9 per cent. for each of the autumn months; and 6 5 per cent. for the winter months, December, January, and February. The mortality from tuberculosis was 2 per cent. less than in the preceding four years, during which the author met with 1317 cases of tubercular disease in 4547 post-mortems, or 28 9 per cent. The per-centage of tubercular disease was found to be much more favourable to females than to males. Dr. Willigk found that in a total of 2114 males examined after death, 35·6 per cent. were tubercular; while of 2433 female subjects inspected, only 23·2 per cent. were tubercular. This closely corresponds with the per-centage established by Dr. Boyd, in his report of the pauper lunatics at the Marylebone Asylum—viz., 36 per cent. males, and only 21 per cent. females. A curious fact, elicited by Dr. Willigk's analysis, is that the number of male victims to tuberculosis varies less at different periods than that of females; among the latter, fluctuations to the extent of 5 per cent. were observed at Prague, whereas the greatest variation among men was 1·4 per cent. Thus, in 1852 and 1853, when above 26 per cent. of all females brought to the deadhouse had succumbed to puerperal fever, 29 per cent. (after subtracting the puerperal cases) were found to be tubercular; whereas in 1854, when puerperal fever presented a much milder form, only 24·6 per cent. were found to have died of tuberculosis.

The following observations, bearing on the cure of pulmonary tuberculosis, deserve attention. In the year 1851–55, Dr. Willigk met with 81 cases (in 302) in which tubercular disease had undergone a curative process in the form of obsolescence, or chalky conversion; 40 occurred in male, 41 in female subjects. Taking the whole of his 1255 cases of pulmonary tuberculosis, Dr. Willigk met with this conversion in 309 cases, or 24 6 per cent. The largest number of cures were found during the epoch at which the largest number of deaths occur—viz., between thirty and forty years. This process is more frequent among females than males.

The post-mortem examinations of cholera subjects present no feature of special interest. From the author's remarks on the puerperal cases, we extract the observation that 43·1 per cent. exhibited the puerperal osteophyte of Rokitansky.

The number of cases of cancer amounted in five years to 177, or 10·5 per cent. Among the males, the number of deaths from cancer progressively increased from the tenth year to the eightieth at the rate of from 2 to 6 per cent. for every decennium; whereas the females presented the greatest mortality from this cause between fifty and sixty, after which the numbers rapidly decline. If cancer of the sexual organs be eliminated, the preponderance is actually on the side of the male sex by 2 per cent., which depends upon the predominance among them of cancer of the stomach and bones. Dr. Willigk has met with four cases of bone-cancer, in which the morbid product had been spontaneously separated, leaving clean and partially-healed ulcers. He has met with one case of scirrhus recti, in which a spontaneous cure was demonstrable.

From the analysis of the apoplectic cases, of which 208 occurred in five years, we extract the remark bearing upon the frequency of cure. Dr. Willigk found cicatrices (43·3 per cent.) or cysts (56·7 per cent.) in 97 instances. This favourable relation loses its value in part by the circumstance that the majority of the individuals, soon after the cure of the local process in the brain, died of diseases of the respiratory or digestive organs, which, from their frequent coincidence with apoplexy, may well be designated as sequelæ of that affection.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E., London.

I. *On the Treatment of Nævus by Vaccination.* By M. Legendre. (Archives Générales. May, 1856, p. 513.)

M. Legendre believes that the ill success which has attended this practice in the hands of some, is chiefly due to the defective mode adopted. He sums up his observations as follows:—

1. *Choice of the Lymph.*—It is of the greatest importance that all the vaccinated spots should take, as it is from their multiplicity and confluence the inflammatory process results sufficing to transform the erectile tissue into non-vascular cicatricial tissue. When but two or three from among seven or eight punctures succeed, the number is usually insufficient for the production of the requisite amount of inflammation, while it prevents the repetition of the operation. The lymph should be therefore taken directly from the arm of the child that supplies it, the lancet being charged afresh after each puncture, and the operation performed slowly, so as to involve only the superficial lymphatic net-work of the skin.

2. *Number of Punctures.*—There is nothing fixed with regard to this, depending as it does upon the size of the nævus; and while one nævus may require seven or eight insertions, double this number may be necessary for a very extensive one. It may be laid down as a general rule, that a sufficient number of punctures must be made to admit of the edges of the pustules, after their complete development, running into each other. M. Pigeaux states that this end will be attained by making the punctures at the distance of a centimètre from each other.

3. *Place of Vaccination.*—Most authors direct the inoculations to be made in the erectile tumour itself, and not at its circumference; but this practice not infrequently gives rise to hæmorrhage, which alarms the friends. It is generally very difficult to make several punctures in the excessively thinned skin of an erectile tumour, without piercing the erectile tissue, especially as it is impossible so to control the movements of the child, as to be certain that the lancet will not penetrate farther than we desire. Notwithstanding this inconvenience, direct inoculation must be resorted to whenever the nævus is situated on the face; for if we vaccinated around its circumference, the ensuing cicatrix would be larger than the tumour itself. When the nævus is out of sight we need not mind this, and by vaccinating near to, without implicating the erectile tissue, we avoid all danger of hæmorrhage, while we can produce a circle of pustules that entirely surrounds the tumour. The erectile tissue more and more invaded by the increasing pustules, diminishes in size, inflames, and becomes connected together with the pustules into a large, dry, blackish crust. When this falls off, the place of the nævus is found to be occupied by a smooth cicatrix, which is either quite white or scattered over with a few red isolated spots, the size of a small pin's head, and devoid of elevation, the further development of which is prevented by the surrounding cicatricial tissue.

M. Legendre points out the desirableness, before vaccinating infants, of inquiring whether any erectile tumour exists, in order that the opportunity of so treating it may not be passed over.

II. *On Urinary Calculi and Lithotomy in Egypt.* By Professor REYER, at Cairo. (Wien Mediz. Wochensch., 1856, Nos. 14—17.)

Professor Reyer states that calculous diseases are of such frequent occurrence in Egypt, that practitioners awhile since were accustomed to reckon their lithotomy operations by hundreds. Although competition has now divided practice, he has still in five years had to perform lithotrity nine, and lithotomy fifty-six, times. He has collected one hundred and twenty-eight calculi, of all of which a careful chemical examination has been made. This prevalence of the disease has been attributed by some to the great concentration of the urine that ensues on profuse sweating, erroneously, for the disease is rarely met with in Upper Egypt, and not at all among the negro race. The urine of the Arabs, notwithstanding the paucity of their flesh diet, is very rich in nitrogenous materials; and uric acid, urate of ammonia, and oxalate of lime are often found in it, prevailing in the same ratio as catarrh of the bladder, which is endemic, and usually dependent, as shown by Dr. Bilharz, upon the presence of the *distomum hæmatobium* in the tissues of the bladder..

The *composition* of the 128 calculi is stated to be as follows:—The nuclei in 69 consisted of uric acid, in 2 of urate of ammonia, in 15 of oxalate of lime, in 1 of ammonio-magnesian phosphate, in 4 of uric acid with urate of ammonia, in 22 of uric acid with oxalate of lime, in 8 of urate of ammonia with the phosphates, in 3 of urate of ammonia with oxalate of lime, in 2 of these two last and uric acid, in 1 of oxalate of lime and phosphates, and in 1 of the ova of the *distomum*. The considerable *predominance* of uric acid (combined usually with more or less oxalate of lime) was observed in 63 out of 110 calculi; the predominance of urate of ammonia was observed in 9, of oxalate of lime in 24, and of the phosphates in 10. Uric acid, in more or less considerable proportions, was found in 102 calculi; and oxalate of lime (often in slight proportion) in 107. In only 8 out of the whole collection did the calculus consist of but a single ingredient; viz., in 1 of uric acid, and in 4 of oxalate of lime. In 42 there were two constituents, in 37 three, and in 41 four. No example of only phosphatic formation was known at Cairo, and in one instance only was the nucleus so composed. Among 25 calculi collected by Dr. Schledehaus in Alexandria, several consisted of phosphates only—a difference which the author attributes to the greater frequency of inflammatory irritation of the bladder in the wet and changeable climate of Alexandria. In the author's cases more than one stone was met with in 13 instances, or 16 per cent. Seven in his collection weighed above five ounces, the heaviest being seven and a half and six and three-quarters. The great bulk of the cases occurred between the ages of twenty and forty.

Professor Reyer performed lithotomy fifty-six times, and lost nine cases, the operation almost universally resorted to at Cairo being the *median*, as practised by Vacca. A sound, having a very broad groove, is introduced, and held perpendicularly by an assistant. The operator feels for the groove through the soft parts by means of the nail of his forefinger, which has been allowed to grow, and having found it, he thrusts the point of a slightly convex scalpel directly into the groove, at about an inch and a half above the anus. He passes the point for about half an inch along the groove towards the bladder, and then, depressing the handle, brings the blade out in the mesial line so as to leave a wound of more than an inch long in the perinæum. The scalpel is laid aside, and the left forefinger is passed into the wound, having the free surface of the nail looking towards the perinæum, and serving to guide into the groove of the sound the button end of a long lithotomy knife, which is trenchant along its anterior third. The finger is now withdrawn, and the operator, taking the sound in his left hand, depresses it somewhat as if he desired to pass it deeper into the bladder, raising the handle somewhat towards himself in order to bring the urethra nearer the arch of the pubis. At the same time he passes the button end of the knife along the groove into the bladder. The left finger is now again introduced into the bladder, the

sound withdrawn, and the forceps introduced along the volar surface of the finger.

Reviewing the different stages of this operation, the opening into the urethra so high up, anterior to the bulb, first calls for notice—the Arab practitioners choosing a point still nearer the scrotum, owing to the greater ease with which the groove can there be felt. The division of the bulb would be feared in Europe, on account of hæmorrhage; but in the author's fifty cases operated upon by this median plan there was little bleeding, while secondary hæmorrhage, easily arrested, only occurred in three cases. Hæmorrhage after any operation is rarely met with among the Egyptians, and the author has never seen it occur after amputation. Professor Schuh, performing this median operation in three cases at Vienna, met with alarming hæmorrhage in two of them. In children and fat persons dissection of the soft parts of the perinæum has to precede opening the urethra, as the piercing it through all these would be uncertain.

In the second stage of the operation the extent of the incision of the prostate is of chief importance, as this, except in the case of very large stones, should be small—one or two lines sufficing for a stone weighing three or four ounces. A too considerable incision might extend through the fibrous covering of the gland, and expose the patient to the greatest of all dangers, that of urinary or purulent infiltration, while the rectum would be more liable to be injured. The cases which have best succeeded in the author's hands have been those in which, in stones of a medium size, the prostate has scarcely been divided. In the author's fifty operations the rectum was wounded in three accidentally, and in one intentionally, on account of the size of the stone.

In the third stage the author lays great stress upon the importance of the information derivable by the introduction of the finger prior to that of the forceps, in regard to the size, form, and position of the stone. To acquire this, when the neck of the bladder is placed very high, considerable pressure has to be exerted over the pubis. While guiding in the forceps, care should be taken not to direct the volar surface of the finger downwards, so as to bring the forceps between this surface and the anterior wall of the rectum, for in that way the cellular tissue between the prostate and rectum may be easily torn. The difficulty in extracting the stone chiefly depends upon the amount of resistance offered by the prostate, this yielding sufficiently for small stones. In larger stones its tissue is torn in one or more places; but as these ruptures are limited by its extensible covering, infiltration does not occur. When the prostate is hard and thick, much force is required even for the extraction of medium-sized stones, while even a large stone can be removed easily when the gland is thin and soft. The author removed a six-ounce calculus with remarkable ease, the prostate having diminished in size. Breaking of the stone during extraction occurred in seven out of the author's fifty-six cases, an accident that may happen in spite of every care on the part of the operator, owing to the loose texture of the calculus. Four of these patients died, and three recovered; and he strongly disapproves of the practice, except when quite unavoidable, of breaking up large calculi in the bladder, and removing them in fragments through the wound. In a case of very large stone, the author preferred inducing a recto-vesical fistula, in order to obtain room for the extraction.

III. *On a New Mode of Reducing Strangulated Hernia.* By Baron Seutin. (Bull. de Thérap., tome l. pp. 161 & 206.)

Baron Seutin declares, that with his mode of reducing strangulated hernia, which he has now practised for twenty years, he hardly ever in his large practice finds it necessary to have recourse to an operation.

The patient is laid upon his back, with the pelvis raised much higher than the shoulders, in order that the intestinal mass may exert traction upon the herniated portion. The knees are flexed, and the body is slightly turned to the opposite

side to that on which the hernia exists. The surgeon ascertains that the hernia, habitually reducible, cannot be returned by continuous and moderate taxis. He next seeks with his index finger for the aperture that has given issue to the hernia, pushing up the skin sufficiently from below, in order not to be arrested by its resistance. The extremity of the finger is passed slowly in between the viscera and the herniary orifice, depressing the intestine or omentum with the pulp of the finger. This stage of the procedure demands perseverance, for at first it seems impossible to succeed. The finger is next to be curved as a hook, and sufficient traction exerted on the ring to rupture some of the fibres, giving rise to a cracking very sensible to the finger, and sometimes to the ear. When this characteristic crack is not produced, the fibres must be submitted to a continuous forced extension, which, by distending them beyond the agency of their natural elasticity, generally terminates the strangulation. This mode of procedure is more applicable to Gimbernat's ligament, the hooking and tearing of which are more difficult than in the case of the inguinal ring. Considerable strength has sometimes to be exerted, and the index finger becomes much fatigued. When, in consequence of the narrowness of the ring, the finger does not at once penetrate, it is to be pressed firmly against the fibrous edge, and inclined toward the hernia. After a time the fibres yield and the finger passes. When the finger becomes fatigued it is not to be withdrawn, but it should be supported by the fingers of an intelligent assistant, who seconds the action it is desired to produce. In inguinal hernia, the traction should not be exerted with the finger upon Poupart's ligament, but in a direction from within outwards, and from below upwards, by which the aponeurotic layers between the two ligamentous pillars constituting the inguinal aperture are easily torn through.

The ring is then enlarged by this tearing, just as if it had been divided by a cutting instrument, or largely dilated, and reduction takes place easily, by performing the taxis in a suitable direction. The mobility of the skin, its laxity in parts where hernia prevails, and its extensibility, greater in proportion to its thinness and to the absence of a lining of fatty cellular tissue—by allowing the sliding and the thrusting of this membrane in front of the finger it cushions, affords protection to the intestine from all immediate contusion. When the strangulation is induced by the issue of a considerable mass of intestine, or an accumulation of fæcal matters, it is desirable first to disengage one of the extremities of the noose, and to seek to expel the gas or fæcal matters by moderate pressure, in order to facilitate the reduction of the tumour. In the few cases in which the finger cannot be introduced, a small incision may be practised in the skin, and the handle of a spatula or any blunt instrument may be passed in by separating the cellular tissue. Pressing this against the border of the ring, while avoiding the intestine, this orifice may be eroded or dilated without danger. The greater the resistance offered by the aponeurotic fibres, the greater will be their tension, and the more easily will their laceration be produced.

As a general conclusion, it may be laid down, that the facility and promptitude of this procedure, and the immunity that attends it, ought to diminish the gravity of the prognosis of strangulated hernia, by rendering the circumstances under which recourse need be had to an operation quite exceptional. Such exceptional cases will be found (1) in old, irreducible herniæ. (2.) When the strangulation in inguinal hernia occurs at the internal ring. Generally the external ring and inguinal canal are large, and allow of the easy penetration of the finger; and then the new method is applicable, and the rupturing or dilatation of the internal ring should be attempted, and the manœuvre is rendered the easier by the fact, that in these cases the canal is much shortened, and the two rings much approximated. If, however, the external ring is too narrow to admit the finger, an operation is required. (3.) When there are general symptoms of a gangrenous state of the intestine.

IV. *Perchloride of Iron in Panniform Keratitis.* By M. FOLLIN. (Archives Générales, April, 1856, p. 424.)

M. Follin, after adverting to the obstinacy of chronic keratitis, and to the frequent inefficacy of the various means proposed for its relief, states that he has found in the employment of the powerful astringent, the perchloride of iron, a most useful application, and that he has had his views of its utility confirmed by MM. Broca and Gosselin. He does not recommend its employment in a high degree of causticity, and believes that at 30° Baumé it is best suited for this purpose. He lets fall a large drop into the eye, by means of a quill, every second or third day, the great contraction of the eyelids that ensues rendering it necessary that all should be introduced at once. It imparts a yellow colour to the eye, and gives rise to a sense of painful constriction, which lasts for about a quarter of an hour, and then gradually diminishes, a burning sensation still continuing for some hours. A slight phlegmasia is sometimes induced the next day in the eye; but however that may be, the perchloride must be abstained from, and cold and slightly astringent applications—among which, the decoction of rhatany is a good one—must alone be resorted to. The perchloride is to be used again on the second, third, or fourth day after, according to the amount of irritation remaining from the former application; and generally it is after the second or third application that decided amelioration is perceived. The perchloride has never given rise to any accidents; and when its application could not be borne, it has not aggravated the condition of the eye. Several cases are related.

V. *Perchloride of Iron as a Hæmostatic.*

A correspondent of the 'Moniteur des Hôpitaux' (1856, No. 24) states that one of the principal elements of his success in the difficult and dangerous operations M. Maisonneuve is famous for undertaking, is the remarkable use he makes of hæmostatics during their performance. He cites a recent case, occurring in a lad of sixteen, of fungous tumour of the dura mater, the growth of which, after having been temporarily arrested by ligature of the carotid, took on enormous proportions, and was accompanied by exhausting hæmorrhages. M. Maisonneuve determined upon its removal, but the tumour bled on the slightest contact, and the patient would not be able to bear the slightest loss of blood. The line of incision extended from the anterior parts of the ear to the summit of the head, and descending along the nose, was carried backwards, and then upwards to the base of the jaw, and its point of departure. A great number of arteries were thus divided, five or six of which, by reason of their anastomatic enlargements, had acquired almost the size of the radial artery. Intelligent assistants immediately compressed them with the finger, but it was impossible to thus continue the dissection without exposing the patient to the danger of death from syncope. M. Maisonneuve therefore applied to each vessel a little pledget of charpie soaked in perchloride of iron, which was allowed to attach itself to the wound. At every stroke of the bistoury or scissors he applied a new plug, so that during the operation the patient scarcely lost a spoonful of blood; and when the tumour had been entirely removed, the entire surface of the wound was found completely dried and tanned, and was at once dressed, without the necessity of the application of a single ligature. The brown eschar which covered the wound was detached about the 20th day, without giving rise to any hæmorrhage; and although the cure can scarcely be expected to prove radical, the patient for the present is perfectly well.

VI. *On Gangrene from Arteritis*. By PROFESSOR PORTA. (Omodei Annali di Med. Feb. 1856, p. 416.)

The following are some of the conclusions arrived at by Professor Porta, from the observation of thirty-one cases of his own, and the consideration of those published by others:

Although the tunics of arteries consist of tissues little disposed to inflammation, yet are they not exempt from liability to it; and external violence, the extension of phlegmasia from other tissues, rheumatism or metastasis may induce an arteritis that may lead to gangrene of subjacent parts. Among all these causes, metastasis is pre-eminent, so that eighteen out of the thirty-one cases are referrible to it. Not infrequently, on the decline or disappearance of some serious internal malady, a reverberation is directed to the arteries of the limbs, the original disease either then disappearing, or remaining as a complication of the newly-developed arteritis. The large external arteries, such as the axillary, humeral, femoral, or popliteal, are usually the subjects of such reverberation, but it has not as yet been met with in the carotid. Exceptionally, smaller arteries are attacked, such as the radial, ulnar, or tibial.

The end to which arteritis tends is the closure of the artery, all the manifestations observed subsequent to the cessation of its pulsation being but the sequelæ of that. Strictly speaking, however, such cessation of pulsation is not pathognomonic of obliteration, as sometimes a minute stream continues to pass, which excites so feeble an oscillation of the vessel as not to be perceptible to the touch. The obstruction of the artery does not necessarily give rise to gangrene, for not only may it be incomplete, but even when complete, it may have been formed with sufficient slowness to allow the development of the lateral anastomoses; the amount of the obliteration, indeed, exerting less influence than the rapidity with which the coagulum is formed. This local condition is not the sole cause of the gangrene, for the production of this may be favoured by a disordered state of the general circulation, or a temporary enfeeblement of the cardiac impulse. There is, however, no lesion of the function of the capillaries operating, as the minute vessels are found healthy and empty in the midst of the gangrened parts, just as they are in mortifications that supervene upon ligature. Gangrene from arteritis presents a great analogy to senile gangrene, which may take place slowly or rapidly, according to the amount of ossific deposit, and the other conditions of the subject.

There is nothing constant observed as regards the form, extension, or duration of this result of arteritis. Sometimes the patient dies during the prodromic stage, in consequence of the rapid exhaustion of his powers before the limb has mortified. In other cases, there are eschars, limited to the skin, or the gangrene may attack only one or more toes. Frequently, however, it extends to the foot and leg, or the hand and fore-arm, until the power of the lateral circulation restores the equilibrium, if it succeed in so doing. If even it is arrested, there is a disposition to relapse; and a paresis, and temporary or permanent atrophy of the limb, remains. Danger to life, however, is not alone dependent upon the degree of extension of the gangrene, but also upon the general state; this allowing us sometimes to hope for recovery in even extensive gangrene, while at others it renders a limited gangrene a most grave circumstance. So dangerous an affection is it, that few succeed in escaping from its effects.

Besides the internal changes that may exist as the effects of the malady which has also caused the arteritis, we often find in the artery supposed to be affected but slight traces of lesions. In bad cases, however, a sero-gelatinous fluid is found external to the artery, the cellular coat is finely injected, and the proper tunics are adherent to each other, and fragile. Sometimes there is thickening of the cellular tunic, and exudation of puriform matter or plastic lymph, externally to the vessel, affixing it to neighbouring parts. All these lesions are not of frequent occurrence in arteritis; and except in the case of violence, all the coats of the

vessel may present a normal appearance, and they would be so pronounced, were it not for the obstruction caused by the product of inflammation. This consists of a solid coagulum of plastic lymph, varying in size, length, and degree of adhesion to the vessel. Sometimes small coagula are observed obstructing the artery at intervals, but more commonly it is a single coagulum, one or more inches in length, converting the vessel into a cord. Sometimes, however, the coagulum assumes the form of a canal, or presents here and there small lacunæ, containing a milky or semi-fluid reddish matter, which may also cover the whole surface of the coagulum, or almost constitute its entire substance. Maisonneuve and Cruveilhier have found even the smallest vessels corresponding to the gangrened part obliterated; but, for the most part, the closure will be found only in the vessels above the gangrened part, those corresponding to this remaining open—showing that the coagulum has preceded the gangrene.

The principal veins of the limb sometimes participate in the inflammatory condition, and exhibit the signs of this more plainly than do the arteries. Their coats become thickened, and rich in vasa vasorum; while their cavity is filled with lymph, or, oftener still, by puriform matter combined with cruor. In ordinary cases, however, the principal veins remain free, contain a small quantity of blood, in part fluid and in part coagulated, or, without exhibiting any signs of phlegmasia, are obstructed by a sanguineous coagulum.

As the arteritis is unpreceded by any prodrome, no prophylactic can be employed; but in order to prevent or circumscribe the formation of coagula, the arteritis itself must be actively combated by antiphlogistic means, general or local, according to the amount of reaction and the condition of the patient. These must, however, be employed with due caution; for while we combat the inflammatory action, we have to favour the lateral circulation. As soon as the more urgent symptoms are mitigated, aromatic fomentations or warm applications should be made to the part, improving the patient's diet, and even exhibiting stimuli, if not specially contra-indicated. If the pain is violent, opium is here, too, of great use. These means are, however, often of no avail; for the arteritis, especially when metastatic, appears suddenly, gives rise to the exudation, and at once disappears; gangrene following if the lateral circulation cannot resist, and leaving to the practitioner only the office of administering palliatives. So, too, all attempts at dissipating the coagulum are useless, this remaining even in the case of recovery; and all that can be done is to endeavour to limit it by favouring the lateral circulation. Even in the case of recovery, until the circulation is completely re-established, there is great danger of relapse.

VII. *On the Treatment of the Hydrocele of Children.* By Dr. Linhart. (Froriep's Notizen, 1856, vol. ii. No. 4.)

In hydrocele met with immediately after birth, there is usually a wide communication with the abdominal cavity; and as there is frequently a fold of gut at the upper part of the tumour, it sometimes occurs that hernia and hydrocele alternate—so that two practitioners, called at different times, may give different opinions respecting the case. This form scarcely requires any special treatment, since the serum returns, during the horizontal position, into the cavity of the abdomen, where it is easily resorbed. The only treatment likely to be of any use would be the keeping the neck of the processus vaginalis compressed by a bandage.

It is otherwise when the hernia occurs later after birth, when it is tense, and the communication with the abdomen is either very small or absent, the processus vaginalis being closed above. In the first case, the fluid will often return slowly into the abdomen, although it may occupy six or eight days in so doing; and such cases deceive the attendants of the child into the belief that the means employed have produced the resorption of the fluid. The deception is the more likely, as, in very great narrowing of the upper mouth of the processus vaginalis, which is often

more than an inch long, re-position cannot be induced by the taxis. This difficulty of returning the fluid is often mistaken for an impossibility, and unnecessary operations resorted to. Indeed, the diagnosis of complete closure is very difficult. When such closure does exist, the case does not differ from one of ordinary hydrocele of the tunica vaginalis.

The indications of treatment are, the removal of the fluid and the closure of the processus vaginalis. With regard to the first, resorption frequently occurs spontaneously, but it can rarely be influenced by the practitioner. The various stimulants employed for this purpose are inoperative, or may be even hurtful by irritating the scrotal skin. When they seem to have been of avail, an aperture has, in fact, existed. The resorption, however, is remarkably facilitated by a subcutaneous incision of the processus vaginalis, which allows the fluid to become effused into the scrotum, where it is rapidly absorbed. A fold of the scrotum should be raised, and a concave tenotomy knife passed in flat between the scrotal skin and the serous sac, so as to make an incision of from one to one and a half inches in length in the processus vaginalis. Dr. Linhart prefers this to seeking to obliterate the vaginal process by means of pressure applied to its neck, which is either ineffectual or cannot be borne, or to the employment of injections, which at this age are not without danger.

VIII. *On the Influence of Phosphate of Lime in the Production of Callus.* By M. A. Milne-Edwards. (Comptes Rendus, xlii. 631.)

The question of aiding the formation of callus by the administration of phosphate of lime has recently been revived in Paris, and the author of this paper alludes to some experiments tried by M. Gosselin at the Hôpital Cochin, especially in cases of fracture of the arm, which are sometimes so long in uniting. In the six cases observed by him the result seemed satisfactory, inasmuch as the apparatus could be removed in from twenty-seven to thirty days, the fracture appearing quite consolidated. As, however, in these cases, the condition of the callus could not be verified, M. Edwards undertook a series of comparative experiments on animals. Fractures as nearly as possible alike were executed upon dogs and rabbits of the same size and strength, to some only of which the lime was administered. The phosphate employed was prepared by the calcination of bones, and consequently was combined with carbonate. The results were decidedly favourable; and the author believes that the phosphate may be usefully employed as an adjuvant, expediting the union in ordinary fractures, and tending to prevent the non-consolidation of others.

From another communication,* it appears that in one of M. Gosselin's cases of fracture of the lower third of the humerus, complete consolidation occurred in thirty days. He administers as a minimum dose half a gramme per diem.

IX. *On the Diagnosis of Sebaceous Tumours.* By M. Chassaignac. (Moniteur des Hôpitaux, 1856, No. 49.)

M. Chassaignac observes, that when sebaceous tumours occur in unexpected localities, he has often found a useful means of diagnosis in observing the exceeding degree of paleness which their surface presents when the base of the tumour is compressed so as to throw this surface into relief; this being much more decided than is the paleness of surface of any other description of tumour submitted to the same procedure.

* Gazette des Hôpitaux, No. 150. 1855.

X. *On the Treatment of Ranula.* By M. GOSSELIN. (L'Union Médicale, 1856, No. 2.)

M. Gosselin, after alluding to the various modes of treating ranula that have been adopted, and the relapses that are so common after them, describes the plan he has himself found beneficial. He first of all performs excision, as recommended by Boyer, and then cauterizes with the nitrate of silver. Next day he introduces a probe into the wound, owing to its tendency to close, and repeats the cauterization the day after that. On the third or fourth day he enlarges, by means of the scissors, the aperture, which has become too narrow, and on the following days cauterizes again. After ten or twelve days of this assiduous attention, if on the introduction of a probe he finds the cavity is obliterated, he leaves the opening to itself. If, however, a track of a certain extent still exists, he again enlarges the orifice with the scissors. This attention to the case is rarely required beyond fifteen days, when the external opening becomes closed, and the cavity being obliterated, there is no fear of relapse. M. Gosselin has operated in this way in several cases, and in three of these, which he has watched for several years, no relapse has ensued, the opening remaining closed. This plan of procedure has also been extended to various analogous cases, in which there is a cavity with secreting walls, having no spontaneous tendency to approach each other.

XI. *Injection of Balsam of Copaiba in Gonorrhœa.* By M. DALLAS. (Gaz. des Hôp., No. 45, 1856.)

Mr. Dallas, of Odessa, states, in confirmation of the observations already published by Taddei, Marchal, and others, that the injection of balsam of copaiba is the most efficacious mode of treating gonorrhœa. In sixteen cases he has so employed it, using no internal remedy, either in recent or old gonorrhœa, with complete success. His formula is copaib five drachms; one yolk of egg; gummy extract of opium, one grain; water, seven ounces. The injection should be used several times a-day.

XII. *Lupulin in Spermatorrhœa.* By Dr. PESCHECK. (Buchner's Repert. fur Pharm., No. 1, 1856.)

Dr. Pescheck has employed lupulin for several years in a great number of cases in which spermatorrhœa seemed to depend upon no mechanical cause. At first, he used to give two grains night and morning; but finding such doses of no avail, he prescribed from ten to fifteen grains to be taken just before bedtime, prohibiting the drinking of water after it. From such doses, even continued for a long time, he has found no inconvenience to arise, while they have acted beneficially on the disease. In some cases he combined with it one or two grains of pulv. digitalis. A valuable peculiarity in the operation of lupulin, is the beneficial action it exerts upon the digestive process, which so often suffers in these cases. It is also very useful in mitigating the urethral irritation and discharges consequent on former excesses, and in many cases more so than iron or quinine. Its especial utility in the chordee of gonorrhœa, Dr. Pescheck has had many opportunities of witnessing. It is best administered without any additions that might diminish its bitterness, as its effects are very proportionate to the intensity of this property. Old lupulin deprived of its oil and bitter taste is almost always useless.

XIII. *Occlusion of the Eye in Ophthalmia.* By M. BONNAFONT. (Bulletin de l'Académie, t. xxi. pp. 437—524.)

A very animated discussion has recently been occasioned at the Académie de Médecine by a paper of M. Bonnafont's on this subject. He states that at the

military hospital, Du Roule, he has found this treatment more efficacious than any other. A piece of fenestrated and cerated rag, the exact form of the orbit, is applied over the closed eye. Over this, a light pledget of charpie is laid, and the whole covered by a largish disc of diachylon, which is retained by a bandage. The eye remains thus closed during several days. In bad purulent ophthalmia the apparatus has to be removed every day, to prevent the accumulation of pus; but when the conjunctiva is only moderately injected, the dressing need only be changed in from two to four days, at the end of which time the surgeon examines the eye, and determines whether the occlusion should be recommenced. In slight conjunctivitis three or four days usually suffice for a complete cure. A chemosis or ulcerated keratitis requires a little longer time; while one of the cases related shows that complete success may follow this plan in eight or twelve days in ulcer of the cornea, with chemosis and well-marked iritis. It requires some tact to know when to remove the apparatus. If the patient complains of much pain, or if the plaster is distended by discharge, it must be at once removed, and the eye and the surrounding parts well cleansed before renewing it. M. Bonnafont believes the treatment is applicable to *all* varieties of ophthalmia, always taking care to adapt the apparatus very accurately, and to watch its effects closely.

During the discussion at the Academy, M. Bégin stated that he had employed this permanent closure of the eyelids in ophthalmia for a long time at Val-de-Grâce, on the principle of abstracting the inflamed organ from the operation of its habitual stimuli—light, air, and friction of the eyelids. He has found it especially useful in cases of keratitis, that are so often obstinate; in iritis; and especially in the scrofulous ophthalmia of children, when accompanied by excessive photophobia.

M. Larrey stated, that from observations he had made many years since, he had come to the conclusion that occlusion might often be employed with advantage in affections of the eyes, but that he was far from joining in M. Bonnafont's indiscriminate recommendation of it. He thinks collodion a very good means of securing the occlusion, although some patients dislike this, on account of the irritating heat it gives rise to. The occlusion is especially applicable in wounds of the globe, which may give rise to hernia or evacuation, in contusion and concussion of the eye to prevent inflammation and promote absorption, and in passive ocular congestion, as hydrophthalmia. Active congestion and phlegmon, in his opinion, contra-indicate it. He thinks it injurious at the commencement of purulent and other acute forms, but of service when these have reached a stationary or chronic stage. It is useful in conjunctival chemosis, and in aiding the reduction of granulations and varicosities, but it is contra-indicated when any tendency to abnormal adhesions exist. It is especially in affections of the *cornea* that it is applicable, it being as useful in recent wounds as in old ulcerations. It may also facilitate the diminution of staphyloma and the absorption of superficial opacity. All things being alike, it is especially indicated in patients who are refractory to other means of treatment—as children, peasants, conscripts, the insane, &c.

M. Piorry also had long employed occlusion with advantage in *non-purulent* ophthalmias, in wounds of the eye, in procidentia of the iris, after the operations of cataract and artificial pupil, in iritis, in certain neuropathies of the fifth pair, and after the entrance of foreign bodies into the eye.

M. Velpeau did not agree with those who think the air acts mischievously on inflamed eyes, and he denied the utility of keeping patients suffering from these in the dark. The best means of diminishing photophobia is obliging the patient to bear the exposure to air and light; the photophobia then soon diminishing, and the recovery proving more rapid than when the patient is kept in the dark. He has always found that the photophobia was rapidly diminished by such exposure of the eye. Comparing the facts adduced by M. Bonnafont with the results of his own practice, he finds that the mean duration of the treatment is greater under occlusion. Occlusion is, however, of utility within narrow limits, and M. Velpeau has employed it with some advantage in non-purulent conjuncti-

vitis, in some abscesses and traumatic lesions of the cornea, but not with the view of excluding the light, but of maintaining a certain amount of compression, and always using at the same time other means appropriate to the disease in question.

XIV. *On the Comparative Value of Amputation at the Knee-Joint and of the Thigh.* By M. BAUDENS. (Comptes Rendus, tome xli. p. 1077.)

M. Baudens states, in a recent communication to the Académie des Sciences, that the above question is one of those that have engaged his attention during his directorship of the French army in the East. He found that the opinions of all the medical officers whom he consulted, whether in the Crimea, at Constantinople, or the military hospitals at Marseilles and Toulon, were in favour of disarticulation of the knee whenever the amputation of the extremity could not be performed below the patella. And, in fact, the disarticulation of the knee has succeeded in a given number of cases oftener than the amputation of the thigh, even when performed at the lower third. But the disarticulation is only to be preferred upon one express condition—viz., that it be performed immediately after the receipt of the injury. Consecutively, amputation of the thigh should be preferred. This second statement agrees in every respect with all that he has observed, written, and taught during the ten years he has been at the head of the Val-de-Grâce. The excellent results of disarticulation of the knee, especially recorded in his Clinical Observations upon Gun-shot Wounds, were obtained in soldiers who had just been wounded on the field of battle. This difference in the results derivable from immediate and secondary amputation at the knee-joint, depends upon the fact that even in a state of health the size of the bones is not in complete accord with the amount of soft parts—a disproportion that becomes still greater when the patient has lost flesh during prolonged suffering and abundant suppuration.

In another communication, M. Baudens observes that, although the surgeons of the Sardinian army in the Crimea hesitated to employ chloroform, those of the French army have used it in twenty-five thousand cases without any accident resulting. It was always administered with great care, so as not to go beyond the production of insensibility.

QUARTERLY REPORT ON MIDWIFERY.

By ROBERT BARNES, M.D. (Lond.)

Late Physician-Accoucheur to the Western General Dispensary.

I. PHYSIOLOGY AND PATHOLOGY OF THE UNIMPREGNATED STATE.

1. *Obstacle to Menstruation from a Fibrous Membrane capping the Os Uteri.* By M. FOURNET. (L'Union Médicale, January 26, 1856.)
2. *On the Therapeutic Effects of Ergot of Rye and Digitalis in Metrorrhagia.* By M. CARRIÈRE. (L'Union Médicale, March 11, 1856.)

1. A LADY, aged thirty, had suffered at fifteen from measles, which was attended by a very severe peritonitis or metro-peritonitis. At sixteen the first appearance of menstruation. After menstruating pretty regularly for a year, at seventeen each menstruation was preceded and accompanied by great sufferings, which went on increasing ever since; then there were very violent colics, sometimes followed by convulsions, a sensation of swelling and weight in the region of the uterus. These subsided as soon as the blood escaped externally. A marriage of several years had remained sterile. Examined in August, 1855, Dr. Fournet found the os uteri thrown forwards, and covered by a dense fibrous membrane, which adhered

by five-sixths of its margin to the circumference of the os tincæ, where it was continuous with the substance of the cervix. The point of interruption of attachment was marked by a crescentic notch, with a thick border, behind which the point of a probe could be inserted. This membrane was dissected away. The patient was relieved of her dysmenorrhœal symptoms.

2. M. Carrière insists much upon the value of a combination of ergot of rye and digitalis in the form of pills in metrorrhagia. In the cases he relates, the conditions, whether systemic or local, connected with the hæmorrhage, are not noticed. He points out that in order to succeed it is important to attend to the following circumstances:—When there is depression of the pulse, digitalis would keep up hæmorrhage, instead of repressing it; when it is full and strong, this is the case for combining it with ergot. It is to be observed, he says, that the further we recede from the small doses, the nearer we arrive at those which call into action the contractions of the whole organ. This is going too far. Tolerance of the remedies by the stomach is ensured by the trisnitrate of bismuth.

II. Pregnancy.

1. *Case of Tetanus in a Pregnant Woman.* By Dr. Mikschik. (Wochenbl. d. Zeitschr. d Gesellsch. d. Aerzte zu Wien, No 33, 1855.)
2. *On Extra-Uterine Pregnancy.* By J. W. Wilson, M.D. (Indian Annals of Medical Sc., October, 1855.)

1. A girl of sixteen (?), eight months in third pregnancy, five days before admission to hospital, without known cause, was seized with very painful cramp in the fingers and toes of right side. The fore-arm was bent, but could be extended, but returned immediately to former position. No fever, no cephalalgia. Child alive. Eight leeches to nucha. Next day, cramp of right foot quite ceased; frictions with chloroformised oil. Third day, excepting a little stiffness of fingers, cramps quite stopped. Fourth day, in the night former state returned, both sides being affected. Fifth day, same state, with some tension of masticatory muscles; in the evening a tetanic fit, not relieved by chloroform. From the sixth to the tenth days, the tetanic fits recurred with more frequency and violence; on the eighteenth she died during a fit. The Cæsarean section gave a dead child. The abdominal muscles were not involved in the tetanus. No albumen in urine. Nothing found on autopsy. No trace of injury.

2. Dr. Wilson, Professor of Midwifery, Calcutta, relates two cases of extra-uterine pregnancy.

Case 1.—Loocharee, aged twenty-three, admitted September 12th, 1855, had always menstruated regularly till about five or six months before admission. When, as she supposed, about four and a half months gone, she suffered an attack of fever, with pain in the iliac regions. She stated she had partially miscarried. The uterine tumour was felt two fingers' breadth above the navel; a great deal of abdominal tenderness; much pain, but unlike labour. Sanguineous discharge from vagina; os soft, open; diarrhœa. Attempt made to induce premature labour. Uterine sound was passed nearly as high as the navel. Increasing tympanitis; as attempts to relieve tension by drawing off the air and liquid by metal and flexible catheters, introduced their entire length through the os, failed, a trocar was pushed into the left iliac region, where the tympanitis was most marked. A considerable quantity of stinking gas issued, and some ounces of an exceedingly fœtid liquid, but the tumour could not be emptied of liquid or air. A second puncture was made below the umbilicus, where the tumour felt soft and fluctuating; arterial blood, evidently placental, welled up through the canula; slight pressure restrained this. The woman's health was now rapidly declining; a director was introduced

through the open puncture, with the view of enlarging the opening to extract the fœtus and wash out the cyst; but the introduction of the director was followed by a free discharge of arterial blood, readily checked by pressure. As appeared afterwards on dissection, had the incision been carried in the direction intended, the mass of the placenta would have been divided. With the view of introducing the finger to ascertain the position of this, I directed the skin-wound to be enlarged. Death on 2nd of October. *Autopsy:*—The parietal layer of peritoneum was of a dark colour, and thickened. From below the navel it was adherent for about two inches, but a free shut sac was formed over the pubes, where adhesion had not taken place. The true cyst was beneath this, and in it a five and a half or six and a half months' fœtus, in an advanced stage of decomposition, lying in a dirty offensive fluid. The placenta was large, situated anteriorly, and to the left. There were no distinctive membranes. The uterus, of normal size, lay behind, and a little to the right; there was a small opening on the left side, communicating with the sac. The intestines and parts contiguous to the sac were so adherent and matted together, that the ovarian tubes and ovaria could not be traced.

Case II.—Dwya, aged twenty-one, admitted 26th April, 1855. Says she menstruated first at twelve years old, and became pregnant immediately after this first menstruation, and at her full time had an easy labour. Subsequently, for eight years, no return of menstruation. She supposes she is now in her eighth month (natives of Bengal reckon pregnancy by lunar periods). Six weeks before admission, she was much reduced by an attack of cholera (every evidence of spanæmia). No indication of kidney disease. Sounds of fœtal heart, 140; mother's pulse, 120. Child last ascertained alive on 16th of May. On the 18th, she said, "My child is dead, it has not moved for some days." From this time she lost ground. Unnatural gestation not being suspected, attempt to produce premature labour was made, ineffectually. The uterine sound being passed up to rupture the membranes, passed to the usual depth of the unimpregnated uterus, where its progress was arrested. At this time, the child could not be felt through the abdominal parietes, and there was a hard, lumpy substance felt above pubis. Extra-uterine pregnancy now diagnosed. Her full time was complete in the first week of July. The solid tumour became less distinct. On the morning of the 3rd, the abdomen seemed swollen and more tense; and on percussing it, the usual completely dull sound was not returned; the tumour over the pubis was no longer perceptible. She had suffered during the night a paroxysm of shivering, followed by febrile symptoms. An enema given returned with some fæces; and shortly afterwards there was observed a continued dribbling of a turbid-looking fluid from the rectum, similar to what was afterwards found to exist in the sac containing the child. She gradually sank, and died on the 6th. An attempt to relieve the tympanitis and explore the case, made by puncturing the fundus of the uterus, failed of success; as was afterwards ascertained from the circumstance of a thick placenta adhering to the outer part of the fundus of the uterus, and preventing the instrument reaching the interior of the sac containing the child. *Autopsy:* Peritoneum, abdominal viscera, and sac, firmly matted together. The sac occupied the fore part of the abdominal walls to the ensiform cartilage. Uterus of natural size. Surmounting the fundus was the sac, of a dark-grey colour, without any distinctive membranes, its walls adherent on all sides to the contiguous parts before and behind, out of which it seemed formed. It contained a child of apparently eight months' development, and a placenta attached to the exterior of the fundus uteri. Where the ovum was originally detained and developed was not ascertained; but the parts, as they were displayed, had an appearance as if it had lodged somewhere near the fundus, either in the Fallopian tube where it joins the substance of the uterus, or near to this extremity; and that it had in its growth lifted the peritoneal covering from the fundus, the peritoneum being carried out as an investment of the sac. The communication with the rectum was not found.

III. Labour.

1. *Case of Tetanus following Abortion.* By Mr. Bazunjee Dossabhoy. (Trans. of Med. and Phys. Soc. of Bombay, 1855.)
2. *Two Cases of Unnatural Labour in which the Cæsarian Section was performed.* By J. Fayrer, M.D. (Ind. Ann., Oct. 1855.)
3. *Three Cases of Puerperal Convulsions successfully treated by Chloroform.* By M. Fréminéau. (L'Union Méd., 5th Feb. 1856.)

1. Early in the morning of the 24th of April, 1853, a Parsee female, aged twenty-six, mother of four children (the youngest a year and a half old), in the third month of pregnancy, after suffering for several hours during the night from uterine pains, had hæmorrhage come on. The os uteri was dilated a very little. She was treated by dilute sulphuric acid and opium, with cold applications; but the pains and discharge continued throughout the day, and at six P.M. an embryo was expelled. The pains then ceased, and the hæmorrhage merged into the ordinary sanious discharge. On the 30th, difficulty in opening the jaws to the full extent was complained of; but there were no other symptoms of tetanus until the 8th of May, when the abdomen became somewhat hard and tympanitic. No stiffness of any other muscles, except those of the jaws. The pulse continued at 100—small, compressible. She remained in this state to the 17th, when spasmodic contraction of the muscles of the back and lower extremities came on; and there was increased stiffness of the muscles of the neck and abdomen. The pulse was 112, of same character as before. She was treated by hemp, quinine, and generous diet; and recovered by the middle of June—menstruation having occurred at the usual period, but rather profusely.

2. Dr. Fayrer relates two cases of Cæsarian section.

Case I.—A Mussulmanee, aged thirty-five, admitted into the King of Oude's Hospital, Lucknow. Labour arrested. Obstruction was caused by general distortion of pelvis; the tubera ischii closely approximated; the rami of pubes are almost in apposition. The outlet is reduced to a passage so small that one finger can with difficulty be introduced. The woman was much deformed in body from rickets during infancy. Spine curved laterally and posteriorly. Extraction of child, even piecemeal, by natural outlet, would have been impossible. The Cæsarian section was performed under chloroform. Hæmorrhage not very great. Opium, calomel, and salines constituted after-treatment. Death on third day.

Autopsy.—Wound partially united; intestines adherent to each other, to the abdominal parietes, the uterus, and the bladder, by coagulable lymph. The cavity of the abdomen contained a quantity of bloody fluid, with shreds of lymph; and the surface of the peritoneum was also covered with it. The uterus had contracted, but the wound was not completely closed: hence the hæmorrhage into the cavity of the abdomen. The child presented some singular abnormities. The body was in size fully natural; weight, eight pounds: length, twenty inches and a half. The right foot was absent. On the left hand, the index, middle, and ring finger were wanting. The fingers of the right hand all present, but malformed; some of them consisting of only two phalanges. The frontal bone was all or nearly absent; upper lip cleft. A tumour, about three inches above the left eye, appears to be a portion of the brain. The child lived for about twelve hours, sucked some milk, and passed meconium.

Case II.—A Mussulmanee slave, aged thirty, admitted at same hospital, 15th April, 1854. Says she has been in labour eight days. First conception; says she is considerably over nine months. Irritative fever. The outlet of the pelvis is so contracted that the finger can with difficulty reach the os uteri. The rami of the pubes approximated; the sacrum bulges forward, reducing the passage so much, that delivery by natural passage seems impossible. Cæsarian section under chloroform. On making incision through the uterus, the placenta was exposed,

being attached to the anterior surface of the womb; profuse hæmorrhage instantly took place. Without loss of time, two dead female children were removed, attached to a single placenta. The hæmorrhage for a moment was frightful, filling the cavity of the abdomen; it was sponged out as quickly as possible, and the uterus stimulated to contract by pressure. She remained under the influence of chloroform for about two hours. Calomel and opium were ordered at once, and ice kept to the abdomen until it produced shivering; the bleeding then ceased. Next day, vomiting. Third day, death.

Autopsy.—No attempt at union of wound in abdomen. Commencing inflammation of intestines; some watery fluid effused into abdominal cavity; the peritoneum in an incipient state of inflammation; the wound of uterus closed by coagulated blood and lymph; and some coagulated blood in cavity. The uterus had contracted so as almost to close the wound. The inlet of the pelvis was contracted to a diameter of less than three inches each way; the outlet was so narrow that the finger could but just pass through, the sacrum bulging forward, and the tubera ischii and rami of the pubes being in close approximation. The body, with this exception, not much deformed.

3. M. Frémineau relates three cases of puerperal convulsions treated by chloroform, which terminated successfully.

Case I.—A primipara, aged twenty-five, at end of gestation had general cephalalgia, redness of face, vertigo, vomitings. At access of labour, a sudden intense fit of eclampsia, with protrusion of the tongue, which, cut and compressed by the teeth, became so swollen that asphyxia seemed imminent. This state lasted for two days. Delivered of a living child. Symptoms persisted; convulsive fits very frequent. Chloroform inhalations, lasting twenty minutes each, and an injection of chloroform. After each inhalation, the patient fell into a state of complete sedation; then, when a fit appeared imminent, she was again submitted to anæsthesia. In the evening she was much better. This treatment was continued for three days, during which time the fits gradually diminished in frequency and severity.

Case II.—A primipara, aged twenty-four, six months pregnant, had been seized two days with eclampsia when admitted (25th March, 1855) into the Hôtel-Dieu, under M. Piedagnel. Loss of consciousness, dilated pupils, convulsive movements, no albuminuria. Attacks followed by hemiplegia. Venesection, and potion containing twenty minims of chloroform. Next day, another attack, same treatment. 27th, another attack; venesection. From the 1st to 7th April, the consciousness and movement returned; at times, still stiffness in limbs; prolonged baths. The 11th, a violent fit; venesection, and chloroform potion. Patient improved. The 14th, after some annoyance, another violent fit; venesection, chloroform potion. Patient now very anæmic. Question of artificial delivery discussed; postponed. Slight convulsive attacks recurred at intervals until delivery on 13th May. Child living. No attack after delivery.

Case III.—A primipara, aged twenty-four, was delivered at the Hôtel-Dieu, in the night of the 21st-22nd November. For some days before, headaches, vertigo; a fit of eclampsia on the 19th, the day when indications of commencing labour appeared. 22nd, profound coma, dilated immovable pupils, stertorous breathing, and occasional convulsive shocks; chloroform potion. 23rd, better. 24th, coma, but no convulsions. 25th, consciousness. From this time gradual recovery.

(The perusal of these cases leaves the impression that proof is wanting of the influence of chloroform in promoting recovery. The repeated bleedings in the second case may have been the more efficacious remedy. The cases seem, however, at least to show that chloroform may be taken in eclampsia without producing injurious effects; and encourage further research.)

IV. Puerperal State.

1. *Note on Puerperal Fever, as it occurred in the Clinique d'Accouchements of Paris under M. Dubois in* 1854.' (L'Union Méd., 6th November, 1856.)
2. *The Erysipelatous Disease of Lying-in Women.* By D. Leasure, M.D. (Amer. Journ. of Med. Science, January, 1856.)

1. In the months of October and November, 1854, there broke out in the clinical hospital of Paris a certain number of cases of puerperal fever. The invasion of the disease in the obstetric wards was preceded, as almost always happens, by the appearance in a great number of delivered women of gangrene of the vulva and vagina. At the same time there was observed in several new-born children a disease rarely seen under other circumstances,—namely, muguet. The course of the disease, its spread, and decline, are not described, so that several of the most interesting questions in the history of this disease receive no illustration from this communication. Three cases in which autopsies were performed are given in detail.

Case I.—On the 17th of October, a woman was delivered after an easy labour. The same day, slowness in answering and embarrassment in speech were observed; the following night, agitation and delirium; next day, face hot, red, eyes injected, abdomen painful, a violent shivering, lasting fifteen or twenty minutes; bowels freely opened by castor-oil; at night again agitation and delirium. Fourth day, bled to 300 grammes. Fifth morning, patient much worse, respiration irregular, 32 inspirations a minute, pulse, 132; abdomen not very tender on pressure. Calomel, half a centigramme every hour. A large red spot, very painful, on the elbow. Sixth day, she died.

Autopsy, forty-eight hours after death.—Injection of the intestinal peritoneum; no pus or false membranes; a small quantity of limpid serum in the retro-uterine peritoneal sac. Uterine peritoneum injected, especially in front; a small false membrane at point of union of right Fallopian tube with uterus; left Fallopian tube adherent to side and back of uterus, its canals containing a creamy, homogeneous pus; tissue of uterus not inflamed; ovaries, nothing remarkable; marked injection of the meninges, especially at base of cranium.

Case II.—Woman delivered after natural labour on 17th October. Two days after, she complained of a pain in right fore-arm; nothing particular observed there. Third day, pain persists. Fourth day, less pain in fore-arm; abdomen a little painful; pulse, 108. Fifth day, general state bad; pulse 124; great depression. Seventh day, salivation induced by mercurial inunctions to arm and abdomen; seems better, but complains of pain in calves and in course of femoral vein; nothing observed. Eighth day, seems still better. Tenth day, difficult breathing, prostration; died.

Autopsy.—No trace of peritonitis. Uterus, at right angle, in substance of walls, several small purulent deposits, surrounded by pyogenic membranes; these abscesses are of size of nuts; the vessels and uterine sinuses near are healthy; elsewhere, and especially in the opposite angle, the venous sinuses contain small, firm clots. Right kidney contains in its upper part a small abscess. Miliary tubercles in summit of left lung. Right fore-arm: skin red; sub-cutaneous cellular tissue red, denser than natural; muscles violaceous, gorged with blood, thickened, hard; no pus.

Case III.—Woman delivered 16th October. On the third day, she complained of pain in the left fore-arm; there is a little œdema; skin hot. Fourth day, pain much increased; limb held fixed in pronation; pain also in leg; nothing observed. Fifth day: arm in same state; pain in outer side of foot and ankle; pain in median line of abdomen. Mercurial inunction; calomel. Sixth day: abdominal pain continues; arm swelled and painful; redness on outside of painful leg. Ninth day: fluctuation commencing in arm; the place was punctured next day, and pus escaped. Eleventh day: another abscess on back of hand; acute

pain in left side, aggravated by coughing; dulness at lower part of lung; mucous rales, and diminished respiratory sounds, besides ægophony. Thirteenth day: death.

Autopsy, fifty hours after death.—No effusion in abdominal cavity; no adhesions; intestinal peritonæum slightly injected; a small false membrane on broad ligament behind ovary. Uterus: marked greyish granulations on inner surface at seat of placenta; sinuses at this level are coloured yellow by pus, but no purulent liquid in their cavities. Ovaries: the left contains an abscess; the corpus luteum is near this abscess. The lymphatics running from the broad ligaments are filled with pus, swollen. Right ovary holds a little pus; lymphatics scarcely visible on this side; ganglions swollen, red, not purulent; kidneys, liver, and pancreas contain no pus. Thorax: left pleural cavity contains sero-purulent effusion; false membranes on costal pleura; lung compressed, tissue hepatised, and containing small indurated foci and droplets of pus. Pericardium: signs of inflammation.

2. (The communication of Dr. Leasure is of great interest, as illustrative of the pathology of puerperal fever.) During the month of March, 1852, an epidemic erysipelas made its appearance in Newcastle, which seemed to put on features of extreme malignity from the very outset, but few of those attacked survived. In the early part of April the first case occurred in my practice. The case was that of a young woman. The throat seemed the principal seat of the disease for the first five days, when the erysipelatous spot made its appearance on one cheek, from which it spread all over the face, head, and neck, and ultimately proved fatal, after rendering her almost a putrid mass whilst still living. On the 11th of April, while engaged in the attendance on this case, I was called to attend Mrs. S in her seventh labour, a very easy one. About twenty-four hours after delivery she got out of bed, feeling very strong, had a chill, followed by fierce fever; delirium in night. Next morning, abdomen swelled rapidly. Forty-eight hours after delivery, when seen, she was *in articulo mortis.* She died next day. The infant died four days afterwards of malignant erysipelas.

I now, says Dr. Leasure, declined attending any more cases, as I was still attending cases of erysipelas. But on the 6th of August, in the absence of the physician, I took charge of Mrs. ——, in labour with her eighth child, of which she was delivered at 1 A.M. Labour natural; child healthy. She continued to do well until 11 P.M. of the 7th, when severe chills set in. They continued for eight hours. Fever followed; pulse 126; intense pain in uterus; no tumefaction of abdomen; countenance anxious, haggard, with frequent frowns and earnest staring, as if at some strange object; lochia not suppressed, but dark and dirty-looking; tongue natural; thirst; obstinate vomiting. I opened a large vein, intending to bleed *ad deliquium*, but the blood soon ceased to flow. The blood did not coagulate, and resembled some dirty mixture, that looked like anything but blood. It had not even the colour of blood. I gave full doses of opium. She died in thirty-six hours of the first chill. The child died of malignant erysipelas within a week; and the old lady who washed and dressed her for the grave took erysipelas within five days of the time she died, but finally recovered.

Under precisely similar circumstances I was constrained to attend Mrs. —— on the 24th of May. Labour natural, not lasting over three hours. She did very well until twenty-six hours after delivery, when she had a chill. Six hours afterwards I visited her, and found her in very nearly the same condition as No. 2. I determined "to bleed her to death," or break down the disease. I opened a vein and took half a gallon of blood. The pulse was not reduced in frequency (130), but became very soft. The blood did not coagulate, but seemed to be dissolved. She died in thirty hours after the first chill.

At the same time, Dr. J. W. Wallace was attending some cases of malignant erysipelas, and the only two cases of labour he attended were followed by precisely similar symptoms, both dying within thirty hours of the first chill. No cases occurred in the practice of the other physicians of the town.

MEDICAL INTELLIGENCE.

Microscopic Appearances of Evacuations in Yellow Fever, observed by Dr. Blair.

THE following letters from Dr. Blair to Dr. John Davy contain observations of so much importance, bearing upon the pathology of yellow fever, that we have much pleasure in making room for them. We may add, relative to the morbid specimens referred to in Dr Blair's first letter, that Dr. Davy finds their appearance under the microscope to correspond with the description, and that the latter gentleman entertains no doubt of their being portions of vessels.

George Town, Demerara, March 8th, 1856.

MY DEAR SIR,—I beg to enclose for your examination a small fragment of material which was expectorated by a seaman, Thomas Bailly, suffering from yellow fever in the Seaman's Hospital, on the 29th ult. The expectoration at the time of observation was of considerable quantity, amounting probably to an ounce. Some of it had a clear glairy appearance, and some was of rather an opaque white, and of a tenacious consistence. Mixed with this expectoration were several red spots, apparently minute blood-clots. On microscopic examination, the pale portion was found to consist chiefly of epithelium, but no cilia were observed on the cells, which were in general very perfect. Several fragments of broken capillary vessels were found mixed with it. When the red spots were subjected to examination, they were found to consist of bundles of capillary fragments, tinted of a bright pink or crimson, and without blood-corpuscles being present. Under the one-fourth and one-eighth inch object-glass of Ross, several of these capillaries were found to be colourless. I enclose a small portion of this material in tinfoil; and, lest decomposition should injure the specimen before it arrives at its destination, I have mounted a minute portion in Canada balsam, which is also sent. Although it is only a week put up in the balsam, I find that it has lost much of its brightness of colour already. The fimbriated ends are also injured. It would likely have done better in a glass cell preserved in Goadby's solution, but I feared that the thin glass of the cell would have been fractured in passing through the post-office. I hope, however, that between the two samples sent, sufficient may reach you to enable you to form a correct idea of its structure. On the 4th of last month, in the case of a seaman named Morrison (fatal), I for the first time observed the undoubted presence of broken capillary vessels in the excretions of yellow fever. In his case, also, it was first noticed in the expectoration. On all former occasions, epistaxis or bloody expectoration was looked on carelessly, as merely a manifestation of the hæmorrhagic tendency, and nothing was expected to be seen but blood-corpuscles under the microscope. These symptoms were therefore almost unheeded hitherto. On this occasion, however, some turn of thought suggested more particular attention to the subject, and the examination of Morrison's bloody sputa led to important results. I have since found the existence of broken capillary vessels one of the commonest phenomena of the disease. They are to be found sometimes in great abundance in the urine, in the alvine evacuations, in the white vomit, in the flaky sediment of the black vomit, in the bloody exudations and hæmorrhages from the mouth, and even on the blistered surfaces. In the flakes of black vomit, it is sometimes necessary to dissolve off the albuminous matter by a drop or two of liquor potassæ before they come fully into view. I had often seen them formerly in the urine and black vomit, and other fluids eliminated from the subjects of yellow fever; but as in most cases they are colourless and empty when so found, I was wont to set them down as extraneous bodies, and suspected them to be fibres derived from the linen-sheets and towels of the establishment. With this preconceived idea, they were of course overlooked and unrecorded. On turning up some old mounted specimens of "caddy stool" of the epidemic of 1851, I find these vessels still existing in them. The fragments of

capillaries are found generally in single cylinders; I have seen, however, a few branched and bifurcated. Their tendency to break off seems to be at the *beadings*. The fracture is occasionally clean, but generally the broken end is split into filaments. A separation of filaments seems to be the mode in which the fracture occurs; and in many fragments, the length of which will occupy three or four times the field of vision of a half-inch object-glass, several partial fractures may be observed in which the tube at such points is split all round longitudinally, and a perfect sub-division is about to occur. At such points on the outer angle, and at the open ends of the capillary fragments, the *débris* of blood corpuscles is to be seen, and these sometimes form a little dossil which is seen connected with the tube of the vessel by fibrillæ. In the urine, I have seen some of the capillary fragments enveloped in the tube-cast material, but encrusted evidently with flat instead of spheroidal epithelium. I cannot observe in the specimens which I have now sent, any epithelial lining within the capillaries; and yet their calibre, I think, is such as would lead us to expect its presence, were they not diseased. In some of the specimens which I have kept of the same expectoration, epithelial matter is visible alongside of the broken capillary vessels, as if it, as well as the blood, had escaped from their cavities. Finding that ecchymosis of the conjunctiva, epistaxis, and some other hæmorrhagic appearances, are common in yellow fever long before the blood has apparently lost any of its fibrine; and finding that even when black vomit is established and the tongue is smeared with blood, the corpuscles are normal in appearance, I cannot but look on the textural lesion of the capillaries as a primary effect of the yellow fever poison, and as the *cause* of the congestions, ecchymoses, oozings, and hæmorrhages, and all their consecutive mischief. The phenomena of the present minor epidemic also corroborate the view that the poison attaches itself to the mucous membranes in the first instance. Its early effects seem to be local. The system is thereby inoculated, and the poison spreads to all the analogous tissues of the body. A general impregnation of the *circulation* in the first instance would be scarcely compatible with the fact of the slow, steady march of the pathogenic influence through the various organs of the body. I enclose the case of Thomas Bailly, as reported in our hospital case book. It will be seen how his attack commenced like a "common cold"—began in the bronchi, and how it gradually extended to the conjunctiva, mucous membrane of the mouth and fauces, to the liver and kidneys, and its final resolution. I may mention, in "reporting progress," that I have detected the glandular cells of the liver to be a common and very large constituent of black vomit. Their shape and size and tint, and the presence of minute oil-globules beside the nucleus in the epithelium, leave no doubt in my mind as to their identity. In the flakes of the black-vomit sediment, also, there is not much difficulty, with the addition of liquor potassæ, in distinguishing the bile-flakes from the blood-flakes.

I remain, my dear Sir, ever faithfully yours,

D. Blair.

Dr John Davy, F.R.S., &c. &c.

George Town, Demerara, April 24th, 1856.

On the 11th inst., while visiting early in the morning a patient of the Seaman's Hospital, named Nolin, I saw in his basin a few ounces of black vomit, with clear, slightly brown-tinted, supernatant fluid, and well-defined sooty sediment in little flakes. This vomit seemed formed from intermixture of food, drink, medicine, blood, saliva or expectoration. Seeming to be as pure as could be obtained, I carried home with me for experiment a small phial of it. My first experiment was to evaporate a large drop of the sediment and serum on glass slips, in the sunshine. I mounted both in Canada balsam. That of the former has given me a fine specimen, in which, along with numerous loose oil globules, many of the glandular cells of the liver (in which the black vomit abounded) are well preserved. I noticed when spreading out the sediment with needles, in order to render it sufficiently translucent for mounting, that it was somewhat flossy in texture, and showed a reluctance to be subdivided. After mounting these, I dropped a little

of the sediment on a number of glass slips, for the purpose of applying reagents. In dropping it from a wide-mouthed pipette, I found the sediment had a tendency to fall in little separate masses or blebs. They dried in the shade in a few hours. After drying, I found each specimen of sediment encircled with a pellucid ring of dry serum, which had oozed out of the sediment. This under the microscope showed only an amorphous glittering. But when I applied a drop of acetic acid to the centre, and it flowed over the margin, the whole pellucid ring started into view filled with colourless, slightly opaque tubules, in the most beautiful loops and reticulations. I need not say with what surprise and delight I looked on this unexpected vision. The tubules were in two sets—the inner in regular network—their diameter filled one space of Ross's micrometer eye-piece, under the half-inch glass. The external set of tubules were at least twice the diameter of that of the others, and their arrangement was in large open festoons. Some of them seemed terminal and acuminated at one end, and appear to lie in a curtain of basement membrane. Within both sets there appeared numerous minute granules. When the acetic acid was stirred about in the sediment (the coloured central portion), numerous detached hepatic cells were brought well into view, and by tearing up this part of the sediment with needles, I observed that several of the large tubercles permeated the mass As usual, several common capillary fragments were present. After a few minutes, the tubules seemed to dissolve, and the acid evaporated, but on a re-application of the acid they reappeared, although not at all with the former clear definition. To another specimen of the dried sediment I applied a drop of water, and found that by it I could detect the tubules, but they were faintly marked, and might have escaped observation had they not been looked for. These vessels are evidently quite different from the capillaries I have hitherto noticed in the excretions of yellow fever patients; while the latter are generally straight and rigid, or broken off at sharp angles, the others are beautifully wavy, and sometimes duplicate, and symmetrical Alcohol and ether acted peculiarly on these tubercles. A movement was instantly caused among them. The meshes swelled up and unravelled themselves, and showed at the angles of the network that the gyrations preserved the same calibre as the other parts. The ether acted in a very fugitive manner, the field soon being obscured by condensed vapour and the haziness from the diffused fat of the liver cells. The dissolved fat of the alcohol did not offer much obstruction to the light. *Liquor potassæ* brought out the tubules faintly and transiently, probably from dissolving them rapidly, but it at the same time extricated several fine large films of basement membrane from the coloured sediment. After the evaporation of the acetic acid and ether, the tubules became again invisible. But after the evaporation of the alcohol they were still to be observed, but in an abnormal condition, and much less distinct than when wet with that reagent. The specimens acted on by liquor potassæ and nitric acid remained in a moist condition for several days, but without a trace of tubule after that time. In the first instance, the nitric acid acted fully as well as any of the other reagents, and brought out the festoons still more distinctly. But it moreover enabled me to trace some of the tubules into the centre of the specimen, and showed them to be a continuation of vessels contained in the dark material which had floated out while the specimen was drying. But still more important, this reagent enabled me to detect *within* them distinctly liver cells, with their minute oil globules. I think there can be little doubt that these tubules are the radical secreting ducts of the liver disengaged from their attachments (or sloughed off) by that destruction of capillary tissue which I am now satisfied is the essential anatomical lesion in yellow fever. May not these observations throw some light on what I believe is still an undecided point in anatomy—viz., the exact manner in which the bile radicles originate in the hepatic lobules? To me, what I have seen seems a demonstration of the induction of Kiernan on this point. It is true that only granules were visible in the sides of those tubules which had been floated out in the serosity; but may not these have been embryonic

cells? or may they not have been the markings of the site of detached cells—the desquamatory process being common to the epithelial surfaces in yellow fever?

Nolin died after four days' illness. He was unusually yellow for that space of time. After death, I found (rather an unusual occurrence in yellow fever) the gall-bladder nearly empty, and what was in it only a little pale pea-green mucus.

I believe I now understand the source and relations of that alvine evacuation in yellow fever which, in the last epidemic, has been named the "*caddy stool.*" It is generally liquid, like dirty water, with a grey, gritty sediment. This sediment under the microscope shows an abundance of crystalline material, chiefly triple phosphates or uric acid, or both; also, although invisible to the naked eye, numerous oblong plates, of a bright yellow colour, which I have latterly called *bile crystals*. But the colour of this stool is derived from innumerable little amorphous masses, granular in surface, and of a jet black colour. This last material I believe is *carbon*. I have detected this black material and the bile crystals in several thin sections of the liver, in those who have died of yellow fever in the present epidemic; and I therefore infer that this peculiar stool is derived from the liver. I have also noticed that this stool seems most common when the respiratory function is embarrassed—in the pulmonary form of yellow fever. On the 5th instant, a Portuguese boy, named V. de Cambra, died with black vomit, well marked, in the Colonial Hospital. This was an exceedingly interesting case, from many circumstances. He suffered so much in his respiration, that his lips were markedly livid. His dyspnœa and restlessness were so great, that no careful auscultation could be made. His blistered surfaces bled so profusely, that the discharge might properly be called *hæmorrhage from the skin*. Two hours before his death, I examined this blood, and found the corpuscles normal; and I washed a small clot which I took up with forceps from his blistered surface, and preserved it in Canada balsam as a proof of the integrity of the fibrine. This boy's liver and spleen are full of what seems to be identical with the carbonaceous particles of the caddy stool.

Summary of Dr. Blair's Views on Yellow Fever.—Since receiving the previous letters of Dr. Blair, we have been favoured with a communication from him, containing the following remarks, which will doubtless obtain that consideration from our readers which is due to any opinion expressed by so careful an observer:

George Town, Demerara, May 25th, 1856.

The proximate cause of the disease is an aerial poison which enters the system through the mucous membranes, on which it impinges, and to which it becomes attached. In some extreme cases, all the mucous linings of the eyes, nares, alvine passages, and bronchi, are poisoned at once; but more commonly, the first application of the poison is only partial. The primary and specific action of the poison is on the capillary vessels, and this action spreads and extends itself until the large viscera and bladder become affected in varying degrees. The mode of action of this poison on the capillaries is first as an irritant, and it ends by inducing a physical impairment of tissue. The consecutive lesions are, desquamation of epithelium, exfoliation of basement membrane, sloughing of the minute capillaries, and deep erosions. The symptoms are first those of an irritant poison, and afterwards are made up of the composite results of hæmorrhage and the circulation of blood, which has been contaminated by impaired functions of the excreting viscera.

The Nursing Scheme of the Epidemiological Society and the Poor-Law Board.

Our readers may remember that we have on a former occasion drawn attention to the labours of a committee of the Epidemiological Society, appointed nearly two years and a half ago, for the purpose of considering a plan, suggested to supply Nurses to the Labouring Classes in sickness, throughout England. Extensive in-

quiries among medical officers of unions, the masters of workhouses, and the clergy, were made, and led to the final adoption by the committee of the proposition for qualifying female inmates of workhouses to act as nurses. In order to carry it into effect they proposed—

I. That the master and matron of every workhouse shall give such female inmates a routine of occupation, that shall afford them a knowledge of the duties required in the management of the sick.

II. That the medical officer of each workhouse, as soon as he shall consider an inmate competent to undertake the nursing of the sick out of the workhouse, shall certify to that effect.

III. That a register shall be kept at the workhouse of all those who have been certified by the medical officer as qualified nurses, containing their names, ages, certificates, and addresses. This register shall be open to the medical profession, the clergy, and the public at large, as a ready means of obtaining a nurse suitable to their wants.

Although these propositions could be carried out in each union without an enlargement of the powers which the boards of guardians already possess, it was manifestly most desirable that the committee should obtain the sanction and aid of the central Poor-Law Board. At an earlier stage of their proceedings the Board had declined adopting the plan altogether. Certain modifications removed the features which were regarded as offering unnecessary difficulties, and the committee, headed by the Earl of Shaftesbury and Lord Stanley, M P, in April last again waited upon the President of the Poor-Law Board, with a view to urging the propriety of a circular letter being issued by the Board to the different unions, recommending the adoption of the propositions of the committee.

The committee shortly after had the great satisfaction of receiving from Lord Courtenay, the secretary to the Poor-Law Board, a copy of a letter addressed to the Poor-law Inspectors, concerning the employment of the able bodied female inmates of workhouses as Nurses; we have much pleasure in publishing this document:

Poor-Law Board, Whitehall, May 10th, 1856.

SIR,—Adverting to the circular memorandum which the Board addressed to you in February in 1855, in reference to a proposal made by the Epidemiological Society for the training in workhouses of nurses for the poor, and to your remarks upon it, I am directed by the Poor-Law Board to inform you, that their attention has been again directed to the subject, and that they think it desirable to communicate to you, for your guidance, the views which they now entertain respecting it.

The Board are of opinion that any attempt on their part to establish authoritatively in workhouses a general system of training for nurses, would be alike impracticable and inexpedient, and they communicated their opinion to the Secretary to the Epidemiological Society in March, 1855. At the same time, the Board think it not improbable that in large workhouses where a paid nurse is employed, it may sometimes be practicable to adopt a system under which such of the female inmates as may be trustworthy and competent for the work, may be employed in the infirmary and sick wards, not only with the object of acting as assistants to the paid nurses, but also with the view of their being taught by them the duties of a nurse in such a manner, as may subsequently enable them to support themselves by becoming nurses on their own independent account.

It is of course unnecessary for the Board to point out, that this species of employment must, however, be subject to the qualification, that no person should be employed in attendance on infectious cases without her free consent. If such a scheme were carried successfully into effect, it is thought that recourse would be frequently had to the workhouses where it was in operation, for nurses to attend the sick; and it is suggested that a register might be kept of the names and qualifications of those inmates who shall have been thus taught, and who are fit for such attendance.

The Board are accordingly desirous that some such plan should be suggested by you to any board of guardians within your district, in which the arrangements of the workhouse are, or may be made, such as to admit of its being carried into practical effect.

The Board further request that in bringing the subject under the notice of any board of guardians, you will not fail to state the strong sense which they entertain of the evils resulting from the want of a sufficient number of trained and efficient nurses for the poor, and their confidence that the guardians will be ready to concur in any plan by which, consistently with a sound system of poor-law administration, and with the laws regulating the expenditure of the poor rate, their number may be increased.

I am, &c.,

(*Signed*) COURTENAY, *Secretary.*

It will be observed that, although the Poor-Law Board in the foregoing letter in the main adopt the principles of the committee, the Board do not make the introduction of the plan imperative upon the different unions, but leave it to the discretion of the Guardians. It would be most desirable that the medical officers of all unions should interest themselves in the plan proposed by the committee, who, we are given to understand, are now issuing printed forms of the certificates and register, with an outline of the qualifications, according to which the certificates might be awarded to the trained nurses, to the different unions; these forms have been maturely considered by the committee, and are offered as suggestions to those who may not hitherto have devoted any consideration to the subject.*

The Trial of William Palmer.

THERE has probably never been a medico-legal investigation which has so entirely absorbed the interest of all classes of society, as the trial which not long since ended in the conviction of William Palmer. Whether in regard to its bearing upon medical and chemical science, or to the relation between direct and circumstantial evidence, or whether in reference to our social and family ties and all those bonds which knit together a civilized community, this celebrated trial demands our most serious consideration. It is not our intention now to enter more fully into the various topics which force themselves upon the mind in connexion with scientific questions that have arisen in the course of the investigation, and that are still occupying the minds of men. We merely allude to it as a matter too momentous, whether regarded as a sign of the times generally, or as a landmark in the history of forensic medicine, to be passed over in silence. We hope to have an early opportunity of discussing the more prominent points that may appear to merit the consideration of our readers.

Report on the Pathology of the Diseases of the East.—London, 1856.

THE fruits which science and, we trust, humanity will gather from the experience of the late war, are beginning to ripen. One of the first that will claim our attention is the pathological Report drawn up by Dr. Lyons, a civil medical officer, who was specially appointed by Lord Panmure, in April, 1855, to institute researches into the morbid changes exhibited in the bodies of those of our soldiers who fell victims to the diseases that decimated our army in the East. The Report has only reached us at the last moment, so that it would be impossible to express an opinion upon it already. Besides, we are informed that a similar Report may be expected from our medical brethren of the army; and it will doubtless be of

* Any information or papers relating to the plan, may be obtained of the secretaries to the committee, 13, Upper Brook-street, Grosvenor-square, London.

great interest to compare the two, and extract from them conjointly those points which may be expected more particularly to interest our readers.

We perceive that Dr. Lyons states that he arrived at Scutari at the close of April, 1855, when he found "that all but the expiring embers of the terrible epidemics of the past winter had disappeared. To the past (he continues) no methods of pathological research are applicable; and to have been enabled to avail myself of the almost unparalleled opportunities for investigating the nature of disease, which had unhappily been presented at Scutari, I should have been in the East not less than fully two months earlier than the date at which my mission commenced." Moreover, it appears that Dr. Lyons laboured under other disadvantages, which somewhat embarrassed him, and caused a further loss of time; for not until the expiration of more than two-thirds of the period originally assigned for his inquiries, were he and his assistants put in possession of the necessary instruments and appliances for the due prosecution of their labours.

Dr. Lyons was assisted by Doctors Aitken and Doyle, the former of whom, as first assistant-pathologist, has also signed the Report, so that the responsibility appears to be divided between the two gentlemen. After spending some time in the investigation of the disease at Scutari, the due and complete appreciation of the pathological characters of the diseases of the army in the East rendered it necessary for Dr. Lyons to move his quarters to the Crimea.

From a cursory glance at his statement with regard to the causation of the diseases that so much impaired the strength of our army, it appears that Dr. Lyons confirms the opinion that the causes were essentially of a character that might have been avoided or removed, and that they were not inherent in the soil of the country. "It is not," he says, "to be inferred that the climate of the Littoral of the Crimea, on which the Allied armies were encamped, is naturally an unhealthy one. On the contrary, there are some very good grounds for quite an opposite opinion. Considerable immunity has been enjoyed by the Allied troops from more than one form of disease which elsewhere has caused extensive ravages amongst forces in the field. Thus, ophthalmic disease has been almost unknown; lesions of the respiratory organs have been of unusual occurrence; and, with the exception of certain marshy lands on the borders of the Tchernaya, intermittent fevers seemed to have no habitat within the lines occupied by the besieging armies. The more recent experience in the English camps seems very clearly to establish, that, with the realization of the other necessary conditions, respecting moderate duties and fatigues, proper and abundant food, suitable clothing, and protection against weather, the climate of the southern shore of the Crimea is such as to favour the maintenance of a very excellent and satisfactory state of health, even amongst very large masses of troops That an opposite state of things is in great part, if not wholly, due to causes which are probably *not* climatic, may be deduced from a consideration of the much less healthy condition of other troops in the same region of the Crimea."

The instructions under which Dr. Lyons proceeded to the East are conceived in a very comprehensive spirit, and are highly creditable to the physician who may have drawn them up, for though signed by Lord Panmure, the subject is scarcely one to which his lordship can be supposed to have devoted any personal attention. The document is itself an interesting one, and belongs to the medical history of the expedition. For these reasons we give it entire.

"1. You will proceed without delay to Scutari, and report yourself, on your arrival, to the commandant, Lord William Paulet, who will be apprised of the objects of your mission, and instructed to place you in communication with the principal medical officers of the hospitals at Scutari.

"2. You will have the entire and uncontrolled direction of the post-mortem researches on the bodies consigned to you for examination; but you will be required to demonstrate the morbid appearances discovered to such of the medical officers as may feel disposed to attend. In order that these gentlemen

may have the full advantage of your pathological researches, you will arrange with the principal medical officer of the hospital as to the time which will be most convenient to the medical officers to attend your demonstrations.

"3. You will have under your directions two able assistants (one first, and one second), and both accustomed to and versed in the operations of the dead-house.

"4. As morbid anatomy is of little value unless studied in connexion with the history of the disease, you and your assistants will require to visit the hospital wards, in order to become acquainted with the symptoms and characters of the diseases during their progress; but you will not interfere with the treatment of the patients. In making notes of the cases, the name of the patient, and the number of his regiment, should always be stated, as by this means the nature of his duties, and the place where he was first attacked by disease, can be more easily ascertained.

"5. The medical officers of the hospitals will be instructed by the principal medical officers to afford you every facility in visiting the wards; and it is hoped that they will be able to supply you with notes of the symptoms and progress of the more urgent cases.

"6. It is expected that you will not content yourself with the simple dissection of the subject, and the demonstration of the morbid parts, but that you will submit them to microscopical examination.

"7. As you are provided with all the appliances necessary for your researches, it is expected that you will take full advantage of the opportunities which present themselves to prosecute your inquiries in this direction to the fullest extent; and as you are also provided with the means of preserving such specimens of disease as you may deem necessary for the illustration of your researches, you will naturally avail yourself of them, and take the necessary steps for having them transported to England.

"8. Although in all probability you will find at Scutari ample opportunities of completing your pathological observations, you are not to confine yourself entirely to the hospitals in that place; should the information which you receive from reliable sources lead you to believe that more favourable opportunities present themselves in the hospitals at Kululee, or elsewhere, on the Bosphorus, you may use your own discretion in transferring your researches for a time to such hospitals, acquainting the commandant of your intention, and the superintendent of the hospital you propose to visit.

"9. With the view of rendering your researches more complete, it is desirable that you should observe the nature of the diseases of the Russian prisoners; and it would further be desirable that you should visit the French Hospitals, and ascertain the results of the researches of the French physicians in the diseases of their sick.

"10. If, after having completed your researches in the hospitals on the Bosphorus, you should be of opinion that you might acquire additional information, to render your researches more complete and useful, by observing the character and effects of the diseases prevalent in their earlier stages and more acute forms in the Crimea, you are at liberty to apply to Lord William Paulet for a passage for yourself and one or both of your assistants and labourers to Balaclava; upon obtaining his approval, you will, upon your arrival, report yourself and explain the object of your mission to the head of the medical department of the army in the Crimea.

"11. Should your health unfortunately suffer so as to render you unable to continue your researches, you are to commit the charge of carrying them out to your first assistant, giving him these instructions as his guide; and if he should require further assistance, application may be made to the principal medical officer at Scutari, or elsewhere, for such assistance, or to the superintendent of any of the civil hospitals in the East.

"12. In the event of either or both of your assistants being unable, from

sickness, to render you the assistance you require for the successful prosecution of your researches, you are to apply to the principal medical officer of the hospital where you may be for one or more assistants, or to the superintendent of the civil hospitals in the East.

"13. If, unfortunately, both yourself and your senior assistant should be attacked by sickness, the circumstances should be immediately communicated to the principal medical officer at Scutari (or elsewhere, or to Dr Parkes, superintendent of the civil hospital on the Bosphorus, who has one assistant-physician at least capable of carrying out the pathological researches on which you will be engaged.

"14. As the office to which you are appointed is new in the medical department of the army, it is possible, although not probable, that some difficulties may arise in the prosecution of your researches. If, by your own prudence and conciliatory conduct, you fail to overcome any such difficulties, you will apply to the commandant of the hospital at Scutari or elsewhere; and should he not afford you the support which you require, you will report the circumstances to the Secretary of State for the War Department with as little delay as possible. You will, however, under any circumstances, report from time to time to the Secretary of State for the War Department the progress made in your researches.

"15. When you shall have completed these researches, you will draw up a full report thereof, for the information of the Secretary of State for the War Department, to whom it should be addressed.

"16. Although not strictly limited as to time, it is presumed that a period of about four months will be sufficient to enable you to accomplish the object of your mission; as soon as you have so accomplished it, you will return to England without delay, in order to present the report of your researches to the War Department, it being important that no time should be lost in making known this report, as it may be the means of elucidating the nature of the diseases affecting the army in the East.

"17. On application to the commandant at Scutari, a passage will be afforded you in one of the first Government vessels returning to England; and immediately on your arrival you will report the same to the Secretary of State for the War Department.

"18. Should one or both of your assistants desire to remain in the East, and should it prove that his or their services are required, the superintendent of the civil hospitals will be authorized to employ his or their services, if he should think fit to do so. If, on the contrary, either or both should prefer returning to England with you, a passage will be afforded to him or them on application to the commandant. The same option, with the approval of the principal medical officer or superintendent of any civil hospital, may be given to your two English porters, if willing to take the duties of orderlies, or if it be found that they can otherwise usefully be employed.

(Signed) "PANMURE.

"London, April 27th, 1855."